

MEDICAL THESES,

SELECTED FROM AMONG

THE INAUGURAL DISSERTATIONS,

PUBLISHED AND DEFENDED

BY THE

GRADUATES IN MEDICINE,

OF THE

UNIVERSITY OF PENNSYLVANIA,

AND OF OTHER MEDICAL SCHOOLS IN THE UNITED STATES:

WITH AN

INTRODUCTION, APPENDIX, AND OCCASIONAL NOTES.

BY CHARLES CALDWELL, M. D.

EDITOR OF THE WORK.

TO BE CONTINUED ANNUALLY.

PHILADELPHIA:

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District of Pennsylvania, to wit :



BE IT REMEMEBERED, That on the tenth day of January, in the twenty-ninth year of the Independence of the United States of America, A. D. one thousand eight hundred and five, THOMAS and WILLIAM BRADFORD, of the said district, have deposited in this office, the title of a book, the right whereof they claim as proprietors, in the words following, to wit :

‘ Medical Theses, selected from among the Inaugural
‘ Dissertations, published and defended by the Graduates in
‘ Medicine, of the University of Pennsylvania, and of other
‘ Medical Schools in the United States : with an Introduction,
‘ Appendix, and Occasional Notes. By Charles Caldwell,
‘ M. D. Editor of the Work. To be continued annually.’

In conformity to the act of the Congress of the United States, intituled, ‘ An act for the encouragement of learning, by securing the copies of maps, charts, and books, to the authors and proprietors of such copies during the times therein mentioned :’ And also to the act, entitled, ‘ An act supplementary to an act, entitled, ‘ an act for the encouragement of learning, by securing the copies of maps, charts, and books, to the authors and proprietors of such copies during the times therein mentioned,’ and extending the benefits thereof to the arts of designing, engraving, and etching historical and other prints.’

D. CALDWELL,
Clerk of the District of Pennsylvania.

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PREFACE,

BY THE EDITOR.

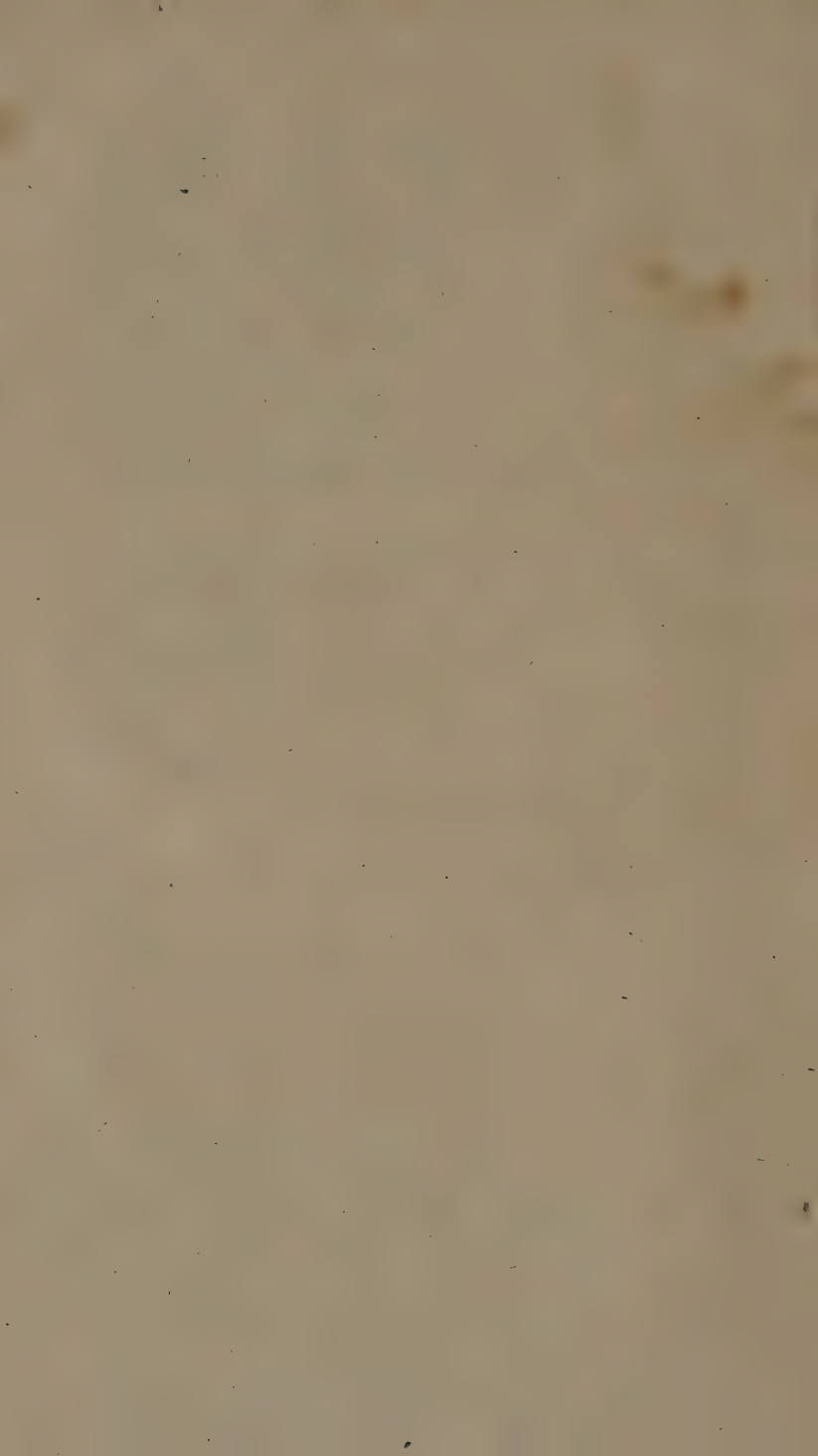
The favourable reception given to the first volume of 'Medical Theses,' published at the commencement of the year 1805, and the very flattering terms in which it has been noticed in several periodical publications, have encouraged the proprietors to proceed with the work. Feeling confident, as they do, that this second volume, is, in no respect, inferior to the first, they can scarcely admit even a doubt, much less can they entertain any serious apprehensions, respecting the estimation in which it will be held, and the fate which ultimately awaits it. Composed, as it is, of various articles, each of them confessedly valuable in itself, it will be easy to form an estimate of its general merit, by viewing it as arising out of the combined excellencies of the whole. The several dissertations which it comprises, are founded on observation—most of them, on experiment, and they are all perfectly accommodated to the present state of medicine in the United States. They do not consist of dry details and recitals of what has been done, said, or conjectured, by medical characters in distant countries, under circumstances materially different from those to which we are subjected. The mere opinions even of celebrated characters, in our own country, are admitted into them with great caution. They exhibit a faithful record of what their authors have themselves witnessed, or of what they have otherwise learnt from sources of the highest authority. Considered as separate specimens of intellect and investigation, they do great credit to the individual writers; and taken together, they constitute a monument peculiarly honourable to the Medical School of Philadelphia.

PREFACE.

From this view of the subject, it is difficult to conceive, how any sincere friend of that school, or any fosterer of medical science in our country, can continue hostile to such a work. If then, any spark of such hostility, does exist, it is hoped that an attentive perusal of the first and second volumes of 'Medical Theses,' will contribute much towards its extinction. The field of medical science, in the United States, earnestly invites, and affords ample scope, for the united labours of all her votaries. It is certainly, therefore, no less the interest, than the duty, of these fellow-labourers, if they even do not deal in acts of mutual assistance, at least to suffer each other to persevere in the great work, without jealousy or molestation.

The editor will only add, that there are already on hand, materials sufficient for a third volume, which will appear early in the year 1807, should the public think proper to continue their patronage,

PHILADELPHIA, FEBRUARY. 1st. 1806.



AN INAUGURAL DISSERTATION:

ON THE

INFLUENZA:

SUBMITTED TO THE EXAMINATION OF THE

REVEREND JOHN EWING, S. T. P. PROVOST;

THE TRUSTEES AND MEDICAL PROFESSORS OF THE UNIVERSITY
OF PENNSYLVANIA,

FOR THE DEGREE OF DOCTOR OF MEDECINE;

ON THE EIGHTH DAY OF MAY, A. D. ONE THOUSAND SEVEN
HUNDRED AND NINETY THREE.

BY ROBERT JOHNSON, OF PHILADELPHIA;

MEMBER OF THE AMERICAN MEDICAL SOCIETY.

"The explaining, correcting and confirming the observations of our predecessors is more useful, and as honourable as hunting after new discoveries, of which the truly learned will find but very few, whatever the ignorant may imagine."——Grant.

DEFINITION.

THIS is a disease capable of being propagated by contagion,* and consists in a preternatural and increased secretion of mucus from the membrane lining the nose, fauces, and bronchiæ, accompanied with a cough, dyspnœa, and a great tendency to sweat.

* In this opinion the editor is obliged to differ from the author. He believes influenza to be a disease *exclusively* of atmospherical origin. Its remote cause is probably a deleterious gas, with the nature of which we are wholly unacquainted. This gas, whatever be its composition or origin, is capable of diffusing itself through a greater extent of atmosphere, and with greater celerity than any other. Hence the wide and rapid spread of Influenza, which sometimes overruns a whole country in a few weeks.

For the production of this disease, an exciting cause is for the most part necessary, which cause, however, never need be very powerful, and at times so strong is the epidemic constitution of the atmosphere, that hundreds are attacked by the disease without having been exposed to any discoverable exciting cause.

All the facts which our author has advanced in proof of the contagion of influenza can be more simply and satisfactorily explained on other principles. Indeed there is no more ground to consider influenza contagious, than there is to annex that quality to intermitting fever or common catarrh.

INTRODUCTION.

THE Influenza seems to have been known to the earliest medical writers whose works we have an opportunity of consulting ; yet, from a variety in some of its most prominent symptoms, it has received a great many different names, and sometimes, been considered as a new disease. Among the writers from Hippocrates to Sydenham, it is generally called *Febris catarrhalis epidemica** ; but in 1673, the latter supposes it to be a *new fever*, and names it accordingly.

It is curious to remark the regular, and constant pace which the science of health and philosophy have kept with each other. As long as philosophers imagined the elements of natural bodies to be four, physicians supposed human bodies to consist of as many humours : but as soon as the corpuscular philosophy became pretty generally received, medicine discovered her ‘acrimony, spiculæ, and salts of various sizes†.’

In like manner, when astrology took the lead of true science, and people began to fancy all terrestrial things were governed by the heavens, some Italian doctors found out that this distemper proceeded from the *influence of the stars*, and therefore gave it the name of INFLUENZA.

Some assert that the disease now known by the name of influenza cannot be the same with that described by Sydenham, as the influenza is manifestly contagious, and the great Sydenham, a strict observer of nature, makes no mention of

* Motherby’s Med. Dict. under the word Influenza.

† Black’s History of Medicine.

any such property belonging to that which he describes. Hippocrates was certainly as attentive, and as sagacious as Sydenham, yet he discovered not (what was much more easily discoverable) the circulation of the blood; a circumstance at which we justly wonder, that it possibly could have escaped the observation of a person much less attentive and enlightened than he who is with propriety styled the father of the healing art. The sameness of the symptoms, however, indubitably proves the identity of the *Tussis Epidemica* and the modern influenza. Dr. Grant in a letter to doctor de la Cour, says that he has carefully attended to the beginning, progress, and termination of the influenza, and finds upon comparison that the same has been discussed by the English Hippocrates long before, '*in as masterly a manner as the subject does admit of.*'

HISTORY OF THE DISEASE.

As a description of this distemper may be found in various publications, I shall content myself with a short account of it, as it appeared in the city of Philadelphia, in the year 1789.

The influenza made its appearance in the month of October, previous to which, and for some weeks after, the weather was dry, cool, and pleasant. It commonly set in with universal lassitude, with chills, and fever; an acute pain in the head, and eye-balls; not unfrequently a considerable itching in the eye-lids; and some had a swelling and inflammation of the eyes. There were some also who had abscesses formed in the frontal sinuses*.

A sore throat, hoarseness, and a pain in one or both ears, in some cases ushered in the disease; whilst in others it came on with a violent sneezing, followed by a discharge of acrid matter from the nose, which often excoriated and inflamed the upper lip. In some cases the nose dropped blood, nay, in some it ran in streams; and in one case related by Dr.

*Rush's Manuscript Lectures.

Rush in his Lectures on the Theory and Practice of Physic* the discharge in this way amounted to twenty ounces.

Most persons had a bad taste in the mouth, attended with a want of appetite, though some few had their appetite preternaturally encreased. A sore mouth was no uncommon attendant on this disease, and some had a severe tooth-ach, swelled jaws, &c. nor did the tongue entirely escape; for in some it was so swelled as to occasion a considerable defect of speech.

The breast was often affected with acute darting or flying pains; these sometimes became fixed, and suddenly brought on, or were accompanied with a painful and laborious respiration. A cough universally attended this disease, which was sometimes convulsive, and extremely painful; in some it brought on a spitting of blood, and two persons died in the act of coughing ||.

The stomach was sometimes affected with nausea, and vomiting, and in some the disease seemed to fall upon the bowels and was carried off with a diarrhœa—though in general the patients were either costive or regular.

The violent pains which in many persons affected the limbs, very much resembled the rheumatism; but those which were felt in the loins and thighs, were remarkably severe. Profuse sweats over the whole body very generally appeared at some time or other during the course of the disorder, and sometimes with obvious advantage.

The pulse was various; sometimes tense and quick, but seldom full. The fever remitted about the fourth or fifth day, but the cough often continued several weeks after every other symptom had disappeared §.

* Which commenced in November 1790, at the college of Philadelphia. It is with pleasure I embrace the present opportunity of acknowledging my great obligation to his valuable lecture on the influenza, for several of the particulars of its history of symptoms here related.

|| Rush's Manuscript Lectures.

§ Hippocrates in his Book of Epidemics has this passage, in which there is a striking resemblance of circumstances, and symptoms: "those who have coughs in the winter, and espe-

The most remarkable circumstances respecting this disorder were the miliary and erysipelatous eruptions which in some instances accompanied it, and the great tendency which the fever manifested to degenerate into typhus. Although it affected persons of both sexes, and of all ages, it was observed that it seized few children below five years of age ; and a physician of this city, whose practice is very extensive, assures me that he remarked *old people* as well as *children*, were less subject to the influenza than persons in middle life.

DIAGNOSIS.

The influenza is not likely to be confounded with any disease except the common catarrh, which, [though it appears with nearly the same symptoms, and like it, often seems to come on in consequence of the application of cold] may be discriminated from it, by coming on with more cold shivering, by the febrile symptoms appearing earlier, and being more considerable in degree. It moreover gives a more sudden and violent shock to the strength, and in many instances produces a perpetual watching, followed by a disorderly and uneasy state of the mind, materially different from the phrenetic delirium of the febris ardens incident to patients labouring under catarrhs from cold, or such like inflammatory diseases.

It is likewise distinguished by its affecting more persons at once, spreading over a greater extent of country in a given time, and in being [more] contagious. The influenza is sometimes accompanied with miliary and erysipelatous eruptions, but the catarrh from cold is not. The latter is slow in its advances, seldom giving alarm until, perhaps, long after the existence of danger, whilst the former, for the most part less dangerous, excites immediate terror, as well by the number of functions which it affects at the same instant, as by the rapidity of its progress ; for in the influenza the transitions

cially with the southerly winds, are subject to fevers during their hawking up much thick matter ; but then they commonly cease in five days. But coughs will extend to forty." Clifton's Hippocrates. Page 214.

from apparently high health to sickness are often, as it were, instantaneous.

Blood-letting, and other suddenly debilitating remedies were seldom so necessary in this disorder as in the common catarrh, and fewer consumptions were the consequence of it, than might have been expected from a common cold among an equal number of persons.

There is no disease to which the human body is liable, so extensive in range, so sudden in attack, so furious at the beginning, so rapid in its course, and at the same time attended with so little danger.

OF THE REMOTE CAUSE.

In a tract of this kind it will probably be expected that the author should inquire into the *original cause* of the disease on which he writes, as such an essay is commonly supposed to exhibit the candidate's medical researches, as well as his medical reading. With regard to this subject, it may not be improper to observe, that it is one which has been considered as very abstruse in every age and country where medical science has been cultivated ; and on which it is not only difficult what to say, but what to leave unsaid. Here I must confess my '*knowledge dwells upon the surface of things* *.'

The present received opinion is, that this species of catarrh arises from contagion, which possibly may be true ; yet to my mind it appears no easy matter to conceive how the disease can spread so far and wide in so short a space of time as we perceive it does, or how it can affect persons many miles apart, at the *same time*, where there had been no previous direct or indirect intercourse—if *propagated only* by 'a matter arising from the body of a man labouring under it.'

Hippocrates when speaking of epidemic diseases in general, says, 'when many are seized with the same disease at one time, the cause is chiefly to be attributed to what is most common and made use of by all. This is certainly what we inhale in inspiration.' Galen is of the same opinion: 'for we all neither are exposed at the same time to other causes,

* Woolaston.

nor are subject to them at all times; but the air alone surrounds us all abroad, and is taken in by all in respiration.'

That the remote cause of the influenza is *chiefly to be sought for in the air*, is rendered highly probable by the following facts. Van Swieten in his comment on the 1407th aphorism of the justly admired Boerhaave, tells us upon the authority of Forestus, that a distemper, which he calls a *malignant catarrh*, 'arose as it were from a certain vapour, since thick clouds of an ill smell preceded it for some days, breaking out so suddenly, that it seized almost instantly a thousand persons.'

About the latter end of the year 1732, and the beginning of 1733, when epidemic catarrhs raged throughout all Europe, we are informed that the like distemper prevailed in Africa, and even persons at sea, though at an immense distance from land, were affected in the same manner †.

The like has been observed much later: for in the year 1780, when the ship *Atlas* left Malacca, there was no epidemic disease in that place; yet upon her arrival at Canton, it was found that at the very same time, that the crew on board the *Atlas* in the China Seas had the influenza, the same disorder raged at Canton.

'On the second of May 1782, the late admiral Kempenfelt sailed from Spithead with a squadron under his command, of which the *Goliath* was one, whose crew was attacked with the influenza, on the 29th of that month; the rest were affected at different times; and so many of the men were rendered incapable of duty by this prevailing sickness, that the whole squadron was obliged to return into port about the second week in June, *not having had communication with any shore*, and having *cruized solely between Brest and the Lizard*.'

About the sixth of May, Lord Howe sailed for the Dutch coast, with a large fleet under his command; *all were in perfect health*; towards the end of May the disorder first appeared in the *Rippon*, and in two days after in the *Princess Amelia*. Other ships of the same fleet were affected with it

† *Memoires pour servir a l'histoire des Insectes*, par Reaumur, page 435.

at different periods; some indeed not until their return to Portsmouth about the second week in June. *This fleet also had no communication with the shore until their return to the Downs*, on their way back to Portsmouth, towards the third or fourth of June †.

To these facts the following passage from the late celebrated doctor Cullen, seems somewhat opposed: this disease 'has seldom appeared in one country of Europe, without appearing *successively* in every other part of it; and, in some instances has been even* *transferred* to America, and has been spread over that continent, as far as we have had opportunities of being informed ||.

From whence the doctor obtained his information I know not; but, as he has not mentioned a single circumstance whereby a person might be enabled to judge of the *accuracy* of it, and as there are objections to the probability of his information being so accurate as to satisfy us that the disease was transferred in the manner he supposes, I am obliged to consider this passage as an assertion not fully supported, either by concomitant circumstances, or subsequent observations; though, at the same time, I acknowledge it to be the assertion of a very respectable, of a very eminent physician. But from the nature of things, his information in its full extent, could not have amounted to more than—that the disease was later in appearing in some parts of Europe than in others; and that it was not only later in appearing in America, than in some parts of Europe, but that the inhabitants of that *amazingly extensive continent* were not affected with it *all at once*. Now what can this prove? That the disease is propagated only by contagion?—Surely not. But granting all that the doctor has asserted to be matter of fact, that the disease has

† Lond. Med. Trans. vol. 3, page 61.

* In the year 1782, the influenza appeared at London between the 12th and 18th, at Oxford in the third week, and at Edinburgh on the 20th day of May. Could the disease have been *transferred* to these three cities in such quick *succession*, by things imbued with the contagion, or by persons labouring under the complaint? But more of this hereafter.

|| Cullen's first lines, vol. 3, page 104.

appeared in every part of Europe *successively*, and has been transferred to America; will this enable us to account for the appearing of it in the island of Bourbon, situate in Africa, at the identical time that it raged in Europe; or explain in what manner it broke out at the same time among persons at sea and on land, where those at sea had not even the smallest communication either with those on land, or with any other person whatever? We must either *deny* the truth of these facts, or admit that the disease is not *always induced by contagion*, according to the common acceptation of that word; that

§ The author had not an opportunity of consulting the first volume of the Medical Communications on the subject of the influenza until several months after this essay had been prepared for the press; and his surprise was not a little upon reading it, as he there found an anticipation of several observations which he supposed had first occurred to himself. But notwithstanding he conceives that much gratitude is due the society for their generous exertions in favour of the healing art, that the design of the work is laudable, and that the publication contains a vast collection of valuable information respecting this wonderful epidemic;—yet cannot help observing that the following part of the ‘Account of the Epidemic Catarrh, of the year 1782,’ appears exceptionable, in as much as it opposes opinion to matter of fact, and substitutes assertion in the room of argument:

‘It is *credibly affirmed*, that the crews of several ships were seized with the influenza many miles distant from land, and came into various ports of England labouring under it; the same thing is said to have happened to ships in the East-Indies, and other parts. A want of precision, or of authentication respecting the circumstances above alluded to, makes it improper to draw any inferences from them.’ Ibid. Page 65,

That the facts are precise will immediately appear upon referring to them, and if *credibly affirmed*, wherefore is it improper to draw any inferences from them! This may be a convenient way of getting over objections which militate against pre-conceived and favorite opinions; but fortunately for science, this mode of *barely denying facts*, is very unsuccessful in producing conviction or of establishing doctrines, in the present state of medical philosophy. Before we were denied the privilege of inferring from them, it would have been proper to have *shown* that they were not *precise*, or *authentic*, or that they *did not apply*. But without even attempting to do this, the compiler endeavours to elude their

is, the disease cannot in these instances, be supposed to have been propagated by personal communication¶.

The foregoing fact respecting the co-incident appearance of the epidemic catarrh in Africa and Europe, first mentioned by Reaumer [who derived his information from the letters of Cassini] is quoted by Van Swieten, in his chapter on epidemic diseases, and must have been known to Doctor Cullen,

force by suggesting distrust; not by argument, but by telling us that ‘without pretending to deny the truth of them, the following anecdote will serve to shew that *great caution is* requisite before they are admitted.’

‘Mr. Henry of Manchester, informed the society, from what he thought good authority, that a ship from the West-Indies to Liverpool, was by stress of weather driven out of her proper course, into a higher north latitude, where her whole crew were seized with the influenza; but wishing afterwards for more accurate information on the subject, he wrote to Dr. Currie of Liverpool, desiring him to make every necessary inquiry into the matter; that gentleman, who took great pains to investigate the affair, at last met with the surgeon of the vessel, from whom he learnt that before the crew were seized with the disorder, they had been off the north of Ireland, and had some communication with the inhabitants of those parts.’

May we not ask if this anecdote proves *any thing in point*? Is it precise? At what time did this communication take place; in the winter, spring, summer, or autumn, and in what year? Were the persons with whom the communication was had, then labouring under the disease; or was it *then prevalent in that part of Ireland*? This anecdote, which was intended to excite in us a distrust of the truth of the facts before alluded to, proves nothing except its own want of *precision, and deficient authentication of circumstances*. For unless it had been *shewn* that the disease prevailed among the inhabitants with whom the crew had this communication prior to, or at that time, the fact only goes to shew that such a communication took place, and subsequent to that, the influenza appeared on board the vessel. But supposing that Mr. Henry was at first mistaken, and that the crew were infected by the inhabitants as the anecdote *indirectly* suggests, this supposition can in no wise affect the authenticity either of the facts before mentioned, or of any other facts whatever.

¶‘But the greatest number concurred in opinion, that the influenza was contagious, in the common acceptation of that word, that is to say, that it was conveyed and propagated by the contact, or at least by the sufficiently near approach of an affected person.’ Med. Commen. vol. I. page 46.

who was unquestionably, well acquainted with the writings of that great man. My own observations, as far as they have gone, are perfectly consonant with this fact; nor can I conceive why the influenza might not arise as spontaneously in America, as in Europe, and there as readily as in the island of Bourbon. The morbid matter exciting the disease must have originated at *some time*, and *some where*; and a cause like to that which gave rise to it in any one country, at any one point of time, might produce it in another country at the same time, under similar circumstances.

It may be objected, that the disease could not have arisen from the air, because the countries here mentioned must experience at any particular time, very different states of that element. This argument may be allowed to have some weight against the supposition of its arising *entirely* from the *sensible qualities* of the air, but *extremely little* against the probability of its taking birth 'from some inexplicable variety of exhalations contained in it, which mixing with our fluids, or by their stimulus disorder our bodies *.' This was the opinion of the deservedly celebrated Herman Boerhaave respecting epidemics in general, and, as far as I have been able to discover, it has not been overturned *by fair argument*, or subsequent observation, at least as far it applies to the epidemic catarrh. This indeed was not the opinion of Boerhaave alone? but also of most of those physicians who were famous for their assiduous attention to the true source of medical knowledge; the operations of nature. The antients 'tis true, were sometimes mistaken, although they studied nature; and the moderns are not, perhaps, less frequently wrong in their opinions, notwithstanding the immensity of their discoveries; not because they do not study nature at all, and study books alone; but chiefly because they study her either too much through the medium of books and pre conceived hypotheses, or with a view to propagate *something new*.

And this is one great reason why we should seldom read the modern systems of physic, unless well armed with 'a great

* Aphorism 1408.

deal of scepticism on the subject.' We may, and indeed we ought to esteem some few of the writings, and opinions of the modern authors, and, with no impropriety, entertain a modest confidence in our own talents for observation ; yet it would be well not to overlook or despise the *medical records of nature* as handed down to us by the antients ; for in these there are certainly many useful remarks which appear to be at present forgotten. Doctor Cullen made war upon the antients, and, unfortunately for our art, with too much success ; for he not only delivered his pupils from the undue influence of great names, and scrutinized the writings of his predecessors with great freedom, but contributed much to render the reading as well as quoting the antient authors unfashionable. He exposed some of the errors of the humoral pathology, but was not always equally happy in substituting truth in the room of them. The desire of being the discoverer of *something new*, and of being thought wiser than our forefathers, has perhaps, in some instances led to the advancement of useful knowledge ; but it has often proved the very hot-bed of error, and warped the judgment of persons the most ingenuous and enlightened.

Many diseases evidently owe their birth to the sensible qualities of the air, ' for with the seasons, the constitutions of men likewise change,'* and though some of these do not become epidemic, yet many of those which do, only become so

* Clifton's Hippocrates. Page 3.—Sydenham says epidemics are admitted or excluded as the sensible qualities of the air favour or oppose them. On the same subject consult Fordyce on Fevers, page 19, &c. Moisture with heat, and sudden changes from hot to cold, by raising much putrid vapour affects the elasticity of the fibres, destroys the fire and vivid circulation of the blood, and dissolves the humours beyond what a healthy state admits. At the very time the surface of the body requires the freest perspiration, the heat of the air makes the proper quantity of cloathing irksome to inconsiderate people ; from whence it happens, that the most putrid effluvia, which should pass through the pores of the skin, are checked, grow caustic, and mix with the blood, while due care is not taken to preserve the juices from corruption by an antiseptic regimen ; and, when they are corrupted, sufficient regulations are not observed for carrying off the disorder, with efficacy or dispatch, by either proper cloathing, detergent medicines, or a suitable diet, &c.

in consequence of such sensible qualities. Russel in his learned treatise on the plague, says, that without the concurrence of a pestilential state of air, the contagion of that disease when imported, even in Turkey, does not spread.

Did the influenza depend upon a *specific contagion* it must *always exist*, or we cannot possibly ascribe it to such a cause. The small pox, the venereal disease, &c. never intermit; but the influenza has become extinct, and again broke forth upon the world after a period of more than four-score years.†

I do not assert, nor do I wish to be understood to mean, that the influenza is not at all contagious: on the contrary, I am possessed of facts* which prove in the most incontestible manner, that it may be, and often is propagated from one person to another by means of contagion. But I mean, and the arguments which I have adduced, I trust, will warrant the conclusion, that the disease often does arise from ‘*some vicious quality of the air*’† or exhalation in it, as well as from a matter arising from the body of a man labouring under disease.‡

† It is recorded that a similar disease appeared in 1510, 1557, 1580, 1587, 1591, 1675, 1709, in the latter end of 1732 and in the beginning of 1733, in 1743, 1762, 1767, 1775, 1782, and in 1789. See Lond. Med. Trans. Vol. 3. Page 77.

* The following communication I received of Doctor Leib my preceptor in medicine: ‘In the year 1782, when the influenza ravaged the sea coasts of Europe; the ship I was on board of captured a Spanish brig which had been taken by a privateer belonging to the British with whom we were then at war. All the crew on board the brig had the influenza, and we had scarcely cast anchor in the harbour of L’Orient, which was in a few days after the capture, before the greater part of the ship’s crew were seized with the disease, and myself among the number.’

† Hildanus supposed the cause of the Plague at Lausanne and the neighbouring districts was not only contagion, but also *some vicious quality of the air*. For, says he, ‘the huts of the peasants and poor people were not exempt from the plague, though situated on the highest mountains, and at a distance from each other, and the peasants kept not the least intercourse with one another.’

‡ ‘In some instances it was observed that the influenza did not shew itself in certain places until some one or more arrived at those places either actually labouring under the disease, or coming immediately from other places, whose inhabitants had been affected by it for some days: while in other instan-

Considering the subject in this light, we shall be enabled to account for the progressive virulence* sometimes observable in this epidemic, without any manifest alteration in the sensible qualities of the air. The '*vicious quality*' of it conspires with, and greatly assists the effluvia issuing from the sick, to encrease the malignancy of this distemper.

OF THE PREDISPOSING CAUSE.

The venerable Galen judiciously remarks, *that no cause can affect without a predisposition of the body*†, otherwise all who are exposed to the rays of a summer sun would be seized with fevers, as well as those who use too much exercise, are passionate, or grieved. Moreover, all would fall sick during the dog-days, or die of the plague.' This is a circumstance which requires very little proof to be admitted as true, the observation of every body supplying innumerable facts in its support. I shall therefore adduce but a few, nor should these be mentioned, but to combat the only argument of any importance which has been advanced against the idea of the remote cause of the influenza residing in the air.

Baron De Tott, in his memors, informs us, that 'the plague, which that year carried off a hundred and fifty thousand persons, in Constantinople, was then at its height. Obligated (says he) to direct the workmen myself, many of whom were attacked by the distemper, I had nothing to preserve me from it, but the salubrious smell of the forges, and the precaution of giving directions with the end of my cane. But

ces, very attentive and intelligent observers could not trace any communication between the families first attacked in the towns in which they resided, and other places, where the disease had previously appeared.' Lond. Med. Trans. Vol. 3. Page 60.

* 'It was also remarked that those who were attacked later from the time of the appearance of the disorder commonly had it more severely, and were longer ill, &c. Med. Commun. Vol. I. Page 24.

† See a note by doctor Rotheram in Cullen's first lines, vol. I. page 52. 'The predisposing is that which renders the body liable or capable of being affected by disease when the exciting cause is applied.'

perhaps, what most preserved me from the infection was my never giving myself up to fear, and the melancholy ideas of its ravages present*.

I have chosen this fact from amongst the multitude which might be brought forward, first, because the plague is the monarch of all diseases, the most highly contagious of any which afflict the human species; and, secondly, because the baron here relates a matter of fact, without regard to any particular theory in medicine. He was not preserved from the disease, as he supposes, by the *salubrious smell of the forges*, for that was as common to the workmen who were seized with the plague as it was to him; nor was he preserved by the precautions which he observed in giving directions; so short a distance as the length of his cane ‡ was certainly within the sphere of the activity of the infection. 'Tis true, doctor Cullen is of opinion, that persons may be preserved from the contagion of the plague, by avoiding all near communication with the sick, or their goods, and 'that it is probable, a small distance will answer the purpose, if, at the same time, there be no stream of air to carry the effluvia of persons, or their goods to some distance.' It cannot, however, be imagined but that during the baron's continuance at this place [which was a very considerable time] the wind blew perhaps, in every direction; that, therefore, he must many times have been exposed to a '*stream of air*' passing over the bodies and goods of persons tainted with the pestilence, and that he was not preserved from the infectious miasmata either by the smell of the forges, or by giving directions with the end of his cane; but by, what is more probable, his active life in which body and mind were vigorously employed, by his strict observance

|| Volume the second, page 83.

‡ The effluvia arising from the diseased, received into the ambient air, form a pestiferous atmosphere, more or less impregnated with these effluvia, as it recedes from their source. That contagion is thus communicated in the chambers of the sick, appears from persons being infected without touching the diseased body, or any thing in the room that may be supposed to harbour the infection.' Russel on the plague, page 298.

of temperance, and, by his never giving himself up to the debilitating influence of fear.

In the fall of 1789, the influenza was very prevalent in the city of Philadelphia and its vicinity, and perhaps in many other parts of America. At that time I was seized with it myself, and was so ill as to be confined to bed for two days, nevertheless, the remainder of the family, which consisted of six persons of different ages, and of both sexes, entirely escaped every symptom of the disease*.

Patrick Russel, who resided many years at Aleppo, and who consequently had the best opportunity of *seeing*, and *knowing* what he relates, says, that ‘some exposed every way to the infection of the plague as if invulnerable, remain sound the whole season.||’ I am therefore decidedly of opinion with doctor Cullen, that even ‘the most *powerful contagions* do not operate, but when the bodies of men exposed to the contagion are in certain circumstances, which render them more liable to be affected by it, or when certain causes concur to excite the power of it §.’ ‘May we not, then, safely conclude that there is required a certain state of the system favourable to the action of the remote cause, to render it capable of receiving the infection; and that this remote cause of the influenza may *exist in the air*, and yet every person shall not be affected with this epidemic at the same time, as the predisposed state of the system may not be present in every person at this particular time.’¶

* ‘To others, and those numerous, it was so favourable as only to attack very few in each family.’ Lond. Med. Trans. vol. 3, page 59.

|| Page 305.

§ First Lines, volume the second, page 246.

¶ ‘If the cause lay in the air all must have been seized at once,’—but as this was not found to be the case, and as the disease appeared at different periods in different towns and villages, doctor Hamilton infers that the cause was contagion. But this is inferring too much; for even from his own account of Harpenden, Luton, and St. Albans, it appears that at the first of these places, though it is half way between the two latter, and several miles nearer London than St. Albans, the influenza shewed itself later than in either of the other two places. The manner in which he accounts for this difference

The state of the system which is *necessary* to the formation of the disease may be inferred from the symptoms with which it is accompanied, particularly that ‘prostration of strength, and impaired vigour in all the functions of the body, †’ which almost always attend it. The predisponent causes of catarrhs in general tend likewise to designate it as a state of *more or less debility*. These causes, according to Cullen, are weakness of the system, and particularly the lessened vigour of the circulation, occasioned by fasting, by evacuations, by fatigue, by a last night’s debauch, by excess in venery, by long watching, by much study, &c. &c.*

The influenza being contagious furnishes additional proof. ‘The bodies of men, says the last mentioned author, are especially liable to be affected by contagions, when they are any ways considerably weakened by want of food, and even by a scanty diet, or one of little nourishment; by intemperance in drinking, which, when the stupor of intoxication is over, leaves the body in a weak state, &c. &c.‡

But the following cases related by Doctor Hamilton, clearly develop the matter, and very satisfactorily prove that previous debility is absolutely necessary to the admission and formation of the disease. ‘A boy of about twelve years of age, of a stirring disposition, suffered severely; yet escaped the disease, though the rest of the family had been ill some time, till after bathing with other boys in a river, and *remaining there longer than prudent*, when he was seized the next day with the influenza. We may add to this, that he was a valetudinarian for a long time before, but had lately overcome in a great measure all his complaints.’

of attack in point of time in these villages, forcibly applies in support of the doctrine which he wishes to explode. He informs us that Harpenden is on an eminence, the soil of a light dry nature, when compared to the others; and from hence, with great propriety concludes that the difference arises from ‘*its situation favouring less its exciting and predisposing cause.*’

† Currie’s account of the diseases of America, page 102.

* First Lines, volume the first, page 134. And,

‡ At page 246, of the second volume.

‘ A young gentleman at Luton [continues the same author] about twenty-three, of a volatile turn, and *lately a valitudinarian*, but who, for eight or ten weeks had so far recovered, as to be able to follow his amusements, and who, for this purpose, generally walked or rode, whether the weather was favourable or not, several hours a day, often at the same time indulging himself freely in the glass, *was at last* seized with the epidemic, and suffered severely *.’

OF THE EXCITING CAUSE.

As truth is the object of which I am in search, and *not the pursuit of fame for new discoveries*, permit me once more to quote a passage from the great commentator of Hippocrates : ‘ In our bodies, as it were prepared for disease, *some external adventitious circumstance kindles a fever*, which of itself would not generate a violent disease, yet from the disposition of the body, every one of these is rendered, not the cause of the disease but the occasion§.’ The exciting or occasional cause of the influenza must therefore be that external circumstance which kindles the fever, to wit, the morbid miasma, or contagion which has been considered under the general head of the *remote cause*; though strictly speaking, the remote cause includes both the exciting and predisposing causes ¶.

|| Doctor Hamilton after mentioning that soldiers suffered much from the influenza, owing to their irregular living light cloathing, &c. &c. adds—‘ the delicate also, and the valitudinarian, in all my observations were great sufferers, and still greater in proportion as they were exposed to the vicissitudes of the weather.’ See Lond. Med. Memoirs, from. page 432 to 438.

§ See a note by doctor Rotherham quoted at page 17 ‘ no disease can exist without an occasional cause; yet it is necessary, that at the same time, the state of the body be such as to admit that cause to take effect, or act.’

¶ ‘ Remote causes are of two kinds, viz. the predisposing and exciting, or as it is sometimes called the occasional.’ *ibid.*

THE PROXIMATE CAUSE

Of every disease is that which immediately produces it, and whose removal effectuates the cure *. The proximate cause of the influenza is nearly the same as that of a common catarrh from cold, as appears by the similarity of their symptoms, which differ only in *degree*. According to doctor Cullen, 'the proximate cause of catarrh (whether from cold or contagion) seems to be an increased afflux of fluids to the mucous membrane of the nose, fauces, and bronchiæ, along with *some degree of inflammation affecting these parts*. The latter circumstance, says he, is confirmed by the appearance of the blood ;' and it is this latter circumstance, viz. the degree of inflammation affecting these parts, which appears to me to be the proximate cause itself, and the increased afflux of fluids, a consequence of that inflammation †.

It is unphilosophic to admit more causes than are absolutely necessary to explain the phenomenon ; and equally so to assign that as a cause which is only an effect. Is it not also unphilosophic to combine a cause and its effect, and assign the combination as a cause ?

How is a secreting or exhaling surface induced to discharge a preternatural quantity of a fluid ? Is it not, either by some power which determines to that surface, or by some affection of the surface itself ? What shall we then suppose to be the proximate cause of this preternatural secretion or exhalation ? Without doubt the *power* which determines to that surface, or

* See the note above quoted—and Van Swieten's Commentaries, vol. 1, page 21—'a disease as an adequate effect is the same with its complete or proximate cause, the presence of which supposes the disease, and the absence its removal.'

† At our meals the membrana pituitaria is frequently irritated by sharp mustard, so as to cause the nose to run water; yet, who, in this case, would assert that the proximate cause of this temporary complaint was an increased afflux of fluids to this membrane ? Is not the *irritation* of this membrane the *proximate cause* ? This is what we first attempt to remove, and which we never fail to effect, by inhaling through the nostrils the grateful effluvia of a piece of wheaten bread. Is not this *a case perfectly in point* ?

some affection of it, whereby it is compelled to secrete, or pour out in unusual quantity, and not the afflux of fluids to it. *Remove the cause, and the effect ceases*, is an axiom as old as philosophy itself, and happily applies to the present case. If we remove the extra-power which determines an unusual flux to the secreting or exhaling surface, it will perform its office in the ordinary manner; nor will the same effect fail to take place upon removing the affection of this surface whereby it is constrained to secrete or exhale preternaturally. In the influenza, the power which constrains or compels preternatural secretion or exhalation (for it matters not which) is nothing else than an inflammatory affection of such a surface*.

In that kind of gonorrhœa, which is brought on by venereal infection, doctor Cullen observes, that the chief thing to be attended to is the *inflamed state of the urethra*, a circumstance which is not only *inseparable* from the disease, but ‘*occasions all the troublesome symptoms that ever attend it*.’

Swiediaur, in his excellent treatise on the venereal disease, calls the gonorrhœa virulenta a *local inflammation* of the urethra in men, and of the vagina in women, the discharge being only the mucus usually secreted in preternatural quantity, somewhat changed in colour and consistence, by the stimulus applied to these parts; and in express terms, says, *it is like the discharge from the nose and lungs, on taking cold, where the mucus assumes nearly the same appearance*.

It has already been remarked, that the proximate cause and symptoms in general of a catarrh from cold, and those of the influenza, were very nearly, if not altogether the same, [except in degree] which may lead us to conclude, that as an inflammation of the lining of the urethra brings on a preternatural discharge of mucus from thence, altered in colour and consistence; so, in the influenza, a like affection of the membrane lining the nose, fauces, and bronchiæ [being a similar secreting surface] will be productive of a resembling discharge.

* ‘More fluid circulates through, and is secreted, in a part that is inflamed, than when it is in a natural state.’ *Mother. by’s Medical Dictionary*, under the word inflammation.

A certain degree of inflammation favours a copious flow of mucus from the urethra, and yet a higher inflammation will suppress the running entirely, bringing on severe pains in different parts of the body, with an increased action of the heart and arteries. The like is observable in catarrhs, where a certain degree of inflammation excites a free discharge from the nose, fauces, and bronchiæ; whilst an increased inflammation of the internal surface of these parts not only suppresses secretion there, but is followed by a sense of fullness in one or both nostrils, dyspnœa sicca, and a quickened pulse*. This last, viz. the febrile action of the arterial system, is a natural consequence of inflammation in these parts; for, as F. Hoffman observes, and after him doctor Fordyce, ‘any such impediment to the freer circulation of the blood, as destroys its equilibrium, is the essential character of a fever §.’ That increased inflammation produces such effects, is confirmed by uniform experience, as may frequently be seen in the patients affected with gonorrhœa, who use too astringent injections; for in this case they have the running checked, with an aggravation of every inflammatory symptom, seldom failing to bring on inflammatio testium, cystitis, or both, and an immense accumulation of misery.

Sydenham remarks, when treating of the epidemic fever and cough of 1675, that it ‘frequently proved very fatal to abundance of the common people, who, whilst they unadvisedly endeavoured to check the cough by taking *burnt brandy*, and *other hot liquors*, occasioned pleuritic or peripneumonic disorders; and by this irrational procedure rendered this disease dangerous, and often mortal, which of its own nature is slight, and easily curable.’ But it sometimes happened, (continues he) not only when the disease had been unskilfully treated, in the manner above described, but also spontaneously, at the beginning of the illness, or in a day or two afterwards, especially in *tender and weakly persons*, that the cough was succeeded by alternate intervals of heat and cold, a pain in the head, back and limbs, and sometimes a tendency to

* ‘A fever accompanies every inflammation.’ Van Swieten’s Comment. vol. 5, page 81.

§ Fordyce on fevers, page 14.

sweat, especially in the night ; all which symptoms generally followed the fever of this constitution, as it were of the lungs, *which occasioned a difficulty of breathing, stopped the cough, and increased the fever.*'

The obvious tendency of 'burnt brandy and other hot liquors,' taken down in such cases, would be to increase an inflammation already begun ; to stop the cough, or at least the excretion of mucus from the lining of the bronchiæ ; to produce difficult respiration from the swelling of the inflamed membrane ; and, an unavoidable effect, the foregoing increased fever.

The manner in which the disease must necessarily originate, will likewise shew that these symptoms arise from the proximate cause here laid down ; and that the degree of this, accounts for every variety observable in the influenza. For, whether the *morbid miasmata* which constitute the exciting cause, be emitted from the body of one who has the disease, or be engendered in the atmosphere, or exhaled into it from putrefying substances, animal or vegetable, or in short, in whatever manner they may get there—it cannot be questioned, out that they *float in that element* *, and enter with it in inspiration and deglutition ; and being retained by the tenacious mucus of the nose, fauces, lungs, stomach and intestines, irri-

* 'We have many examples to prove, that the air cannot hold, nor yet convey contagion to any distance. If it be mixed with atmospheric air, it is soon dissipated, perhaps chemically decomposed, if it be a compound body [*possibly he would have been nearer the truth had he said re-combounded, or neutralized*], and its nature altogether changed.' Lond. Med. Mem. vol. 2, page 439. Upon first reading this passage, I doubted whether the author meant seriously, as it appeared to me to be trifling, if not with *common sense*, at least with *common experience*, and with the testimony of some of the greatest authorities in medicine. See the quotations from Hildanus, Russel, &c. at pages 16 and 18—'It is well known the stench of putrid carcases, gangrened limbs, the polluted stinking air of jails, &c. bring on malignant pestilential fevers, just as the putrid sanies of a gangrened limb, absorbed into the blood, brings on a fever of the same kind.' Huxham on fevers, page 243. See likewise on the same subject, Ferriar's Med. Essays, p. 236.

tate and inflame these parts, thereby producing, in the first instance, or secondarily, the train of symptoms which take place in this disease. Doctor Houlston of Liverpool, goes so far as to assure us, 'that in sitting near an infected person, an irritation of the mucus membrane of the nose was sensible, such as is produced by the dust of pepper, and which sneezing tended to remove. *

Now, though it seems *almost certain* that the virus of every disease which is contagious, affects the part on which it has first fastened, before it disturbs the rest of the body, and assimilates to itself more or less of the humours which it there meets with; yet it would seem *possible* that some of it might be absorbed, and immediately taken into the circulation [in persons of lax habits,] and there excite or increase a fever by its own stimulus, or by the stimulus of such part of the blood as it assimilates to its own nature †. But for my own part, I should suppose it a *rare occurrence in the influenza*,

* Med. Commun. Vol. I. Page 57. See also James' Med. Dictionary under the word Catarrh. 'But it is not to be doubted, that there is sometimes in the air such a *subtile caustic matter* which, being received in inspiration, insinuates itself into the glandulous parts, through which it passes, excites pain, tumor, and redness, and brings on a catarrhus fever.' What this subtile caustic matter is *essentially*, may no doubt be very difficult to explain; but from analogy it would seem probable that it is the same with that which produces the plague, the jail or hospital fever, and, peradventure, an intermittent: and that the various appearances of these (seemingly different) diseases arise from the greater or less concentration of this *matter*, together with the accidental, though greatly diversifying circumstances of season, soil, cultivation, climate, &c. and also the manner of living, food, raiment, &c. &c.' According to Doctor William Fordyce, 'if animal bodies are in a decaying state, and the air be filled with their steams, they sometimes produce *pestilential fevers*; the steams of some decayed vegetables have the same effect. The effluvia of human bodies are likewise very hurtful to the air. Three thousand men living within the compass of an acre of ground would make an atmosphere of their own steams seventy-one feet high, which would soon become pestilential, without the winds to dispel it. The air of prisons for this reason produces mortal fevers.' See his Inquiry into the causes, &c. of fevers, page 16.

† See Ferriar's Med. Essays, page 235.

that the, *materies morbi*,^{*} in the first instance, enters the mass of blood without exciting a *local affection*; but as there are said to be some cases where the patients are instantaneously seized with, and exhibit all the other symptoms of the disease, we may, perhaps admit that in those cases, the local affection may not be present. However, such cases have *never* fallen under my observation.*

Upon the whole, therefore, the proximate cause of the influenza appears to be a local inflammation of one, or more of the parts before mentioned, viz. of the mucous membrane lining the nose, fauces, aspera arteria, œsophagus, &c. &c. †

OF THE CURE.

So moderate is the influenza in many instances, as to require but a few days refraining from the use of animal food, to lay in bed or keep within doors, taking at the same time some warm diluent drink, and to return gradually to the usual manner of living; whilst in others, again, great attention is absolutely necessary, and the cure difficult.

The treatment of this disorder must be either *local*, or *general*, or *both*; as will appear by attending to its history of symptoms, and its remote and proximate causes: but as most local remedies produce general effects, and general ones often relieve particular parts, it may be most proper to omit distinctions of this kind, and premise one *universal rule* by which the indications of cure are to be governed, viz. *the season of the year, the state of the system, and the symptoms present*.

* ‘I believe contagious miasmata seldom, if ever, produce their effects by entering the vasa inhalantia on the surface of the body where the cuticle is not removed. I apprehend they more commonly make their way by the primæ viæ, the lungs, or other external passages, &c. &c.’

Dr. Kirkland.

† ‘Some inflammation I will allow, says Doctor Hamilton, *the state of the mucous membrane* proved that there was a *degree of it present*.’ Lond. Med. Mem. Vol. 2, page 456. After enumerating certain remedies which he used, adds—‘with a linctus to mitigate the *burning heat and pain* I felt in my throat;’ from which one would think the *degree of inflammation* in his own case was not very inconsiderable.

1. BLOOD-LETTING is a proper and speedy remedy to take down the phlogistic diathesis, and may be either partial or general, according as the symptoms indicate. The pulse, though it may assist in determining the quantity of blood which should be drawn, and the *frequency* of the operation, it can by no means be allowed to direct us altogether in the use of this valuable remedy.* From the great disposition which this fever discovered to degenerate into typhus † we should be cautious in the use of the lancet, and all things else being equal, bleed less freely in the spring than in the fall of the year.

2. CATHARTICS, or purging medicines, are no doubt necessary, particularly if the patient be afflicted with a violent head-ach, a throbbing of the temporal arteries, much cough, constipation of the bowels, accompanied with a tense pulse: but as medicines of this kind debilitate the system considerably by a *single operation*, if given in full dose, it would seem safer [for the reason suggested under the preceding head] to administer them so as to keep the body regular, or gently lax ‡; or to supply their place either by clysters ||, or emetics in the manner next to be mentioned.

* 'Where there is just reason to fear a contagious malignity in a fever, we should proceed with the utmost caution as to repeated bleeding.' Huxham on fevers, page 238. See the 1st vol. of the Med. Commun. p. 75. Notwithstanding this epidemic [the influenza of 1789] *was visibly of an inflammatory kind*, it would not with us, admit of what is called the antiphlogistic plan.' Currie on the diseases of America, page 323: and at page 103. 'Several were benefited by bleeding; but in general the patients recovered sooner when it was omitted, except when pneumonic symptoms; such as acute pain, and a full or hard pulse indicated it.'

† 'In the course of the disease there frequently appeared unequivocal signs of a putrid tendency.' Med. Commun. Vol. I. page 80.

‡ 'Gentle laxatives were frequently used with advantage in the beginning of the complaint, especially where there was a disposition to costiveness, strong purges do not appear to have been often given; and from general observation respecting the effects of bleeding, there is reason to think, they would in most cases have been prejudicial.' Med. Commun. vol. I. page 38.

|| Wallis's Sydenham, vol. II. page 337.

3. VOMITS. Whenever there appears to be an inflammation of the lungs, which may be known by stitches or acute pains about the chest, these would be improper, as tending to give exquisite and unnecessary pain; and would endanger the rupture of a blood vessel in the lungs, with all its bad consequences, without any probability of their proving serviceable. An *early exhibition* of full vomiting is very proper, in order to bring on a determination of the fluids to the surface of the body *, which not only contributes to the *expulsion of the exciting cause*,|| and thereby preventive of an inflammation of the lungs, &c. but often brings on a salutary perspiration over the whole body; a copious secretion of mucus in the bronchiæ, fauces and nose; and in this, anticipating or assisting nature in her own way in bringing on a mild solution of the disease. It will therefore be best to limit *full vomiting* to the first stage of the complaint, and afterward to supply its place by emetic medicines in small doses, frequently repeated, so as to keep up a pretty constant nausea; for which purpose the gum ammoniac, antimonial wine, or emetic tartar is usually prescribed; and, though any one of them will answer tolerably well, the latter being copiously diluted, and frequently given in small quantity, seems to have been attended with the happiest effects, by rendering the bowels

* Emetics exhibited upon the *first attack* [of the influenza] were evidently of use in relieving the head and breast. Lond. Med. Trans. vol. 3, page 73. 'They do not appear to have been very generally used, [in the epidemical catarrh of 1782] but all who did employ them, concur in opinion, that they were of great service, not only where there was reason to suspect an accumulation of mucus in the bronchial ramifications, but also where they were given chiefly with a view to assist in producing a speedy and copious perspiration.' Med. Communications, vol. I. page 35.

|| 'Before the miasma was fixed and propagated in the body, it was wholly carried off in several patients who kept in bed immediately after feeling the first attack, by a large perspiration. Other spontaneous evacuations, by vomiting, looseness or urine were less frequent, and did not seem to procure such immediate, and great relief, unless they were followed by a sweat.'

Dr. Reimarus, Hamburgh. See Med. Commun. vol. I. page 30.

moderately open, and keeping up a gentle diaphoresis §. 'This medicine administered in this manner, [in the opinion of doctor James Carmichael Smyth] had also a very remarkable effect in bringing on a remission of the febrile symptoms, and in accelerating the termination of the disease.'

However, the *long continued use* of antimonials †, or of nauseating medicines of any kind, is apt at length, to debilitate the stomach so much as to render it, in a good measure, incapable of retaining food, drink, or medicine; and this, perhaps, at a time when they are most needful. These medicines likewise soon loose their sudorific power over the system, and, as Dr. Donald Monro assures us, even James' celebrated febrifuge powders have occasioned such a purging as to *hasten the patients to their graves*.

The MISTURA MUCILAGINOSA ‡, which is very frequently used in the Philadelphia Dispensary, is an agreeable and efficacious medicine in most catarrhal complaints; particularly where the cough is very distressing, and the necessary evacuations have been previously made. A table spoonful every two or three hours, according as the cough, anxiety, morbid watchfulness, &c. are urgent, is the manner in which this excellent remedy is usually prescribed. This mixture possesses several advantages, as well from the medicines which it contains, as the due proportion in which they enter into its composition, and the facility with which its powers may be increased without becoming much [if at all] less agreeable to the patient. The tincture of opium will render it more anodyne in a given quantity, a few grains of tartar emetic will correct the constipating qualities of this, and the whole mixture is thereby more or less laxative; whilst their joint efficacy renders it more powerfully diaphoretic, with

§ 'All attempts to *force sweat* appear to have done more harm than good.' Lond. Med. Trans. vol. 3, page 72.

† 'Large doses of antimonials, or even smaller ones too frequently repeated, have sometimes brought on evacuations, which entirely sunk the patient.' Lind on hot climates, page 261. And,

On the same subject, see Dr. D. Monro's Observ. vol. 2, page 13 and 15—also Dickinson on fevers, page 115.

‡ R. Elixir: Paregoric: ʒj Vini Antimonial: ʒss Mucilag: Gum Arabic:—Succ: Clycirrh: aa ʒis—aquæ Fentis ʒviij M.

scarcely any alteration in its taste, or diminution of its dimulcent quality. But I have seldom seen it necessary either to vary the form of the prescription, or give any other opiate*, and I have observed at least a thousand instances since I attended the practice of the above institution,—where the use of it was attended not only with evident relief, but [as the patients sometimes emphatically expressed it] with ‘*blessed effects*.’

4. **LOW DIET.** Animal food seems to be very hurtful, especially in the beginning of the disease; it ought therefore to be immediately laid aside, and a light vegetable or milk diet substituted in its stead.† Doctor Rotherham is of opinion that ‘an abstinence from all food would accelerate the cure;’ and possibly, in some cases, it might have this effect; yet as the prescription seems a harsh one, and might in many persons induce an irritation from hunger much more dangerous than the stimulus of a small quantity of bland aliment in the stomach, it would be preferable to allow as much as would allay this sensation. Low diet has its limits; nor should it be much longer persisted in than whilst the inflammatory diathesis is present in the system.

A gentleman of the faculty in this city, who had the influenza in the fall of ’89, strictly adhered to the antiphlogistic regimen, and to his astonishment perceived the disease, instead of abating, to grow worse: he reversed the plan, lived generously, and got well §.

* ‘Opiates were a common remedy with most physicians; and they all agree in testifying their great use; particularly in mitigating the cough, which was in many cases the most troublesome and tedious symptom of the disease. Med. Commun. vol. I, page 38.

† What doctor Sydenham has beautifully said, when pointing out the cure of the quinzy, is strictly applicable here, viz. ‘Meats of every kind, and likewise broths prepared from them, are *sacred*, and must not be touched.’

§ ‘A generous diet [in some instances] was highly conducive to a more speedy recovery, and many bore a more liberal use of wine than is generally given in catarrhs from cold.’ Lond. Med. Mem. v. 2, p. 468.

5. DILUENT DRINKS. Of whatever kind the fever may be, these seem to be indicated. There is a great variety of them, and but little preference; as any of them will answer sufficiently well, if a due attention be paid to their temperature and quantity.

As a general rule, *tepid drinks* †, would seem to be safest, as cold ones sometimes do injury in inflammations of the lungs, and in some instances, might check or prevent a salutary perspiration: but as there are certainly some exceptions to this rule, much must be left to the sagacity of the physician †.

Pure water, whey, barley-water, water acidulated with currant jelly, vinegar, lime juice, &c. with or without sweetening, are all very proper drinks in this disease. Whilst an inflammatory diathesis prevails in the system, a little nitre or some such neutral salt may be dissolved in one or other of these drinks, and given with safety and advantage. Typhus and typhoid cases require diluents also; but occasionally, the patients may be allowed wine-whey, wine and water, veal-broth, chicken broth, and pure unmixed wine, according to circumstances*.

6. BLISTERS are frequently necessary in this complaint, and peculiarly so when pleuritic or peripneumonic symptoms become violent; in which case, they are to be placed as

† ‘The drinking frequently of *tepid*, emollient liquors, is a kind of internal relaxing fatus to the primæ viæ, præcordia, &c. which is of no small consequence, especially in inflammations of the lungs, pleura, &c. This was the practice of the antients, who gave little else in fevers, besides their watery diluents, ptisan, or barley-water, hydromel, oxymel, &c.’ Huxham on fevers, page 245.—See Doctor William Fordyce’s inquiry into the causes, &c. of fevers, pages 90, and 180.

‡ Brydone says the Italians use ice and ice-water with great advantage in inflammations of the lungs; but I have not learnt that this practice has been imitated in America, and until experience shall have demonstrated more generally its safety, we should venture on it with some hesitation.

* ‘Proper dilution is unquestionably useful in all fevers, but certainly some require more than barley-water, and lemonade.’ Huxham on fevers, page 245. See also Lond. Med. Mem. vol. 2, page 459.

rectly over the part affected as possible*. When the influenza is attended with ophthalmies, head-ach, or acute pains in the eye-balls, blisters applied over the temporal arteries, to the nape of the neck, or behind the ears, are of *eminent service* §.

7. THE PEDILUVIUM should never be omitted in the incipient stage of this, or of any other catarrhus affection. The patient may sit in water of a temperature somewhat higher than that of his own body †, from five to thirty or forty minutes; taking, at the same time, or very shortly after, a few drops of antimonial wine in a little weak tea. The ease with which this remedy may be procured, being within the reach of every body, the suddenness of its effects, and the little danger attendant on its application, are no small recommendation in its favor. According to Doctor Hamilton, ‘it determines to the surface, encourages a larger share of blood from the head and superior parts, to the lower; is generally followed by sleep, relieves delirium, moderates the cough, and removes sickness at the stomach, from the great sympathy between this organ, and all the parts of the body; but especially with the surface ‡.’

8. WARM AQUEOUS VAPOURS frequently received into the lungs by the breath, constitute a remedy of immense consequence in this disease. Many persons fancy that a little vinegar added to the warm water, improves its virtues consi-

* ‘Blisters seldom failed to relieve the head, and to prevent too great a defluxion on the lungs.’ Lond. Med. Transact. vol. 3, page 73.

§ There has been much contrariety of opinion with regard to blisters: some exclaim against frequent, and, as they term, it, indiscriminate use, others forbid the application of them where there is a putrid tendency only; others admit them but object to certain kinds in certain kinds of fevers,—as for instance cantharides in the jail fever, where they would prefer blistering with the steams of hot water, or sinapisms of vinegar, leaven, &c. Whilst others again, the most celebrated of whom is the late doctor Brown, condemn their use in all cases whatever.

† See Huxham on fevers, at page 12.

‡ See his remarks on the influenza of 1782, in the second volume of the Lond. Med. Memoirs.

derably, the truth of which however has been questioned; nevertheless as the vapour, in consequence of this addition, feels more grateful to the lungs and fauces of some people, as it can do no injury, and as it may coincide with the wishes of the patients, it will sometimes become a duty to prescribe in this way. The method of using or of applying the vapour, is of very little importance; a bason filled with hot water, and the face placed over it, or the steam received through an inverted funnel, will answer as well as, and perhaps better than Mudge's inhaler.

DIRECTIONS TO NURSES, AND ATTENDANTS OF THE SICK.

As the influenza is a febrile disease, and a contagious one also, it will be proper to pay attention to the air of the patient's chamber; as the salubrity of this, which depends no less on frequent ventilation, than upon universal cleanliness, accelerates the cure, and is preventive of relapses. Nor is the temperature of it to be neglected; for although cool air is undoubtedly useful in fevers, yet it is not less so in many cases, to support that degree of warmth which may promote a proper quantity of perspiration. A fire-place is of great use in purifying the air, and in some measure regulating the temperature of it, and where a choice can be had, the sick ought never to be put in a room in which there is not such a ventilator. Let me repeat it, every thing about the patient should be kept clean, and his linen frequently changed; his bed placed some feet from the wall, and no curtains suffered to envelope it; all unnecessary furniture should be removed, and no wearing apparel permitted to hang round the room. If at any time it should be found inadvisable to change the air of his apartment by opening the windows, doors, &c. it will be right to impregnate it with the steams of vinegar*, or with the fumes which arise from brown sugar when thrown on a few coals. The present state of philosophy will not enable us to explain satisfactorily how the healthy change is produced;—whether the steams of the vinegar, or the fumes of the sugar neutra-

* 'Steams of vinegar resist putrefaction by impregnating the air with its powers.' Fordyce on fevers, page 18.

lize, or decompose the morbid impurities issuing from the sick,—or in short how it is effectuated : but it is well we are *certain of the fact*, though we should forever be ignorant how it obtains.

OF PROPHYLACTICS,

in the influenza little can be said with certainty, as we are yet so totally ignorant of the true nature of the *materies morbi* ; but it may perhaps be proper to observe that *equanimity* and *temperance in eating and drinking*, are amongst the best preventatives of all disorders. Temperance is too indefinite a term, however, as that which is no more than *strictly necessary* to one person, might to another be a very dangerous excess ; for which reason more ought to be left to the feelings of the person than to the judgment of the physician. When an epidemic rages, and indeed at all times, changes in the manner of living are dangerous ; but especially so, if they be not gradually made, ‘lest by the change some innovation should happen in the body,’ as saith the great Hippocrates.

OF THE PROGNOSIS.

The influenza has brought on death in persons previously very much debilitated, and paved the way for it in some instances by disposing to dropsy, consumption of the lungs, &c. Yet the united testimony of all the writers upon this subject proves that it is seldom either obstinate or fatal : * and as death or recovery in this disease is marked by no peculiar symptom (that I know of) it has appeared to be useless to enter into a detail of doubtful circumstances. There is no part of our medical researches accompanied with so much uncertainty as is the prognosis of disease ; for, in the language of the poet, ‘*shadows, clouds, and darkness rest upon it.*’

I shall conclude this essay with remarking that, although the influenza is, as mentioned above, for the most part a mild disease, still it is not always without danger ; that if there be a risk in leaving our constitutions to struggle with the com-

* See Med. Commun. vol. 1. page 40. The termination or consequences of this disorder were like every other part of it, extremely various.’

plaint, there is as much—nay, more to be apprehended from injudicious treatment; that while in some cases we fancy we are assisting nature, we should be careful lest we be found contending with her to the great hazard of the patient; that although medicines become the props of sinking life when judiciously administered, yet if dealt out by the rash and the unskilful it is justly to be feared they will be used improperly, in which case they are as dreadful as the *sword of the destroying angel*. By this I mean not to insinuate, that the faculty alone are to dispense medicines,—far from it; but would wish to suggest, in the cause of humanity, the necessity of caution, as ‘bold practice’ borders upon cruelty.

A DISSERTATION,
ON THE
SALUTARY EFFECTS OF MERCURY,
IN MALIGNANT FEVERS:

SUBMITTED TO THE EXAMINATION OF THE
REVEREND JOHN EWING, S. T. P. PROVOST;
THE TRUSTEES AND MEDICAL FACULTY OF THE UNIVERSITY
OF PENNSYLVANIA,
FOR THE DEGREE OF DOCTOR OF MEDICINE;
ON THE TWENTY SECOND DAY OF MAY, A. D. ONE THOUSAND
SEVEN HUNDRED AND NINETY EIGHT.

BY JAMES STUART, OF VIRGINIA:
AND RESIDENT MEMBER OF THE ACADEMY OF MEDECINE OF
PHILADELPHIA.

*Præpetibus pennis ausus se credere cælo:
Insuetum per iter gelidas enavit ad arctos.*

Virg.. Ænid VI.

*Ne mea dona tibi studio disposta fidei,
Intellecta priusquam sint contempta relinquo.*

Lucret. Lib. I.

INTRODUCTION.

IF any medicine from general utility and acknowledged virtues in relieving the miseries of humanity, demand the exclusive attention of the physician, that surely is the one which is the subject of the following essay. Mercury has not only eliminated the venereal virus, humbled the obstinacy of dropsy, broke the enchantment of epilepsy, and subdued an innumerable host of diseases, equally inimical to life, but now compels malignant fever to own its sway.

To do justice to the merits of this hero of the materia medica, and to point out its many excellencies in the cure of every disease in which it has been successfully employed, would require more time than I can at present, employ, and extend this treatise to a length beyond the limits generally assigned to an inaugural dissertation. I shall therefore confine myself to treat only of its salutary effects, in what have been termed *putrid, or malignant fevers*. In executing this, I shall divide the subject into four heads.

I. I shall define the term *Malignant*, and offer a few remarks to prove that all diseases are equally malignant in proportion to the prevailing *Inflammatory Diathesis*.

II. I shall consider the *Modus Operandi* of mercury, when applied to the system.

III. The *different modes of applying* it and the several *means of assisting* the operation.

IV. And last. The treatment of the mouth during a salivation, with the remedies for checking it, and the objections from injury to the teeth and constitution.

If in treating of some of these particulars, the benevolent reader should perceive me stepping aside from the beaten track

of his preceptor, and be inclined to associate his censure with my name, I earnestly solicit his indulgence, and beg him for a moment to suspend his conclusions until he reflects that when the animal economy is under a morbid stimulus, it is in proportion to the force of that stimulus, insensible to all others. Hence the almost astonishing doses of opium, daily exhibited in tetanus, would prove fatal to the same person while under the influence of only the ordinary stimuli of health; he will then, I trust, feel no more offence at the exhibition of twenty grains of calomel every three or four hours in the most violent stages of malignant fever, than that the peruvian bark, which was once dealt out by physicians in doses of twenty grains, with a farcical solemnity and all the mystery of magic or necromancy, should now be administered by nurses in doses of half an ounce.

A DISSERTATION ON MERCURY.

I. DEFINITION OF THE TERM MALIGNANT.

PHYSICIANS from the time of Hippocrates to the present day have agreed in affixing to a certain assemblage of symptoms occurring in febrile diseases, such as a grim cadaverous countenance, great prostration of strength, a disposition to faint on being raised up, petchiæ, vibices, dissolved blood, hemorrhagy from different parts of the body, &c. the term *putrid* or *malignant*. The term *putrid*, originated from a supposition that these symptoms depend on a putrescent state of the fluids, but since modern experiments have proved, that such a state in the living body can never take place, * the term has given place to the less exceptionable one of *malignant*, and lately to the *gangrenous state* of fever. † The term *malignant* probably arose from the ferocious, or, malignant countenance of the patient, observable in this state of fever.

A case is said to be more or less *malignant* in proportion to the violence of these symptoms, and as a greater or less number occur in the same time and patient; but, unfortunately neither writers or practitioners have been so unanimous in their opinions or practice, of the *proximate cause*, or in their *methods of cure* in this state of fever. Hence Sydenham complains of the practice of his cotemporaries, ‘Cum in ææ febres presertim Malignæ dicantur, in quibus intensioris præ ceteris inflammationis gradus conspiciatur, hinc medici se ad usum cardiacorum, et alexipharmacorum nescio quorum con-

* Vid. Cullen’s first lines Sect. 72.

† Dr. Rush’s proximate causes of fever.

tulerunt, quo scilicet per cutis poros expellant, quod somneant venenum (hoc enim est dicendum, nisi malint verbis ludere, quam illud quod potest intelligi, serio proponere) ex quo factum est ut regimen calidissimum, methodumque huic parem, iis morbis adaptaverunt, quæ frigidissima tum remedia, tum regimen, præ ceteris sibi postulabant.'

That *malignant* symptoms depend upon an *inflammatory* diathesis, or great *excess* of stimulus, I infer,

1. From the *same* remote and exciting causes which produce inflammatory fevers, when applied in *higher* degree, producing the malignant state of fever.

Hippocrates in his epidemics mentions a case of putrid bilious fever being brought on from the stimulus of a caustic, and Dr. Boerhaave in his definition of a synochus says 'it has been customary to call that disease a putrid synochus, (i. e. a malignant fever) which arises from the *more violent causes* of inflammation.' And his commentator Baron Van Swieten, under the same aphorism considers, 'a *high degree* of stimulus the exciting cause of all malignant fevers.'

2. From the effects of blood-letting in this state of fever, for an account of which as I have nothing to add, I beg leave to refer to Dr. Rush's defence of blood-letting.

3. From the facility with which the inflammatory and malignant states of fever are *changed into each other*.

Dr. Huck, in his remarks on the fevers of Jamaica says, 'It often depends upon the *manner* in which the patient is treated in the beginning, whether he shall have a yellow or only a remitting, or intermitting fever.'

A case is recorded in the Medical Essays to this purpose, a girl who was afflicted with a tertian, took a draught of spirit of wine, hot ale and ground pepper, and in consequence was seized with a violent continual fever; but, when the continual fever went off, the tertian returned with great irregularities, attended with the most obstinate and malignant symptoms. In further proof of this, I will relate a case that occurred in my own practice. In August, 1797, a young man was afflicted with a quotidian, attended with dysenteric symptoms, and was cured by small bleedings, calomel and opium; a few days afterwards, the quotidian returned without any

dysenteric symptoms; on which he unadvisedly took large doses of laudanum, and drank hot spiced brandy (with a view, as he said, of sweating off his fever) by which means it was accended to a high degree of *malignity*, the dysenteric symptoms returned, and he died yellow with symptoms of a mortification of the intestines, on the fifth day from the commencement of the continual form.

Whilst on the other hand, it is equally certain that the most malignant state of fever may be changed to that state where sily blood occurs, and finally, that may be reduced to the diathesis which constitutes health by no other means than a perseverance in the antiphlogistic remedies. In confirmation of which I refer to authors who have employed blood-letting as a remedy in these fevers.*

4. From all fevers, under *certain circumstances* assuming symptoms of the highest malignity.

The small-pox, for example, in a good constitution and under proper treatment, is acknowledged to be uniformly a mild inflammatory fever; but, by a hot regimen and the abuse of stimulating medicines it may be accended to a grade of malignity equal to the plague. Hence in Minorca, before the nature of this disease was well understood, the most of those who survived an attack, remained blind, consumptive, or lame, with caries of the bones, sordid ulcers, &c. so that Mr. Cleghorn justly considered it to approach in violence the plague †. The same remarks are applicable, only in a less degree, to the putrid sore throat, the pleurisy, rheumatism, gout, measles, influenza, dysentery, scarlet fever, puerperile fever, jail fever, &c ‡. for a particular account of which, I refer to authors who have professedly treated on each of these diseases.

* Cotalus, Sydenham and Rush.

† Cleghorn's account of the diseases of Minorca, page 277.

‡ In Mrs. Jeffery's case, near the New Market, last autumn, symptoms of malignity attended a difficult and excessive discharge of the menses. She was relieved by several bleedings at the arm and the use of calomel joined with frictions of mercurial ointment on the region of the uterus, which excited a gentle affection of the mouth; the blood was at first florid without a disposition to separate, but at the second bleeding became sily.

Galen was long since acquainted with this relation between the *inflammatory* and *malignant state* of fever, as is evidenced by the following observation, ‘that symptoms of malignity or putrefaction only vary as nature overpowers the disease, or is herself overpowered, and that in inflammations she overpowers the disease *.’ This opinion, although not in the language of the present day, is very emphatically expressed, as in cases of malignity the excitability of the blood-vessels is *prostrated from excess* of force, nature may be said to be overpowered †; while in what are called inflammatory fevers, attended with sizzly blood, &c. stimuli have been applied only adequate to excite the *highest* convulsive action in the blood-vessels, and *less* than sufficient to induce paralysis, rupture, effusion, &c. nature may properly enough be said to overpower, as she is still *capable* of reflecting the force of stimuli.

After what has been said, I shall define a *malignant fever* to be that state of fever, in which there is overaction in the blood-vessels, or a defect of action and a disposition to paralysis or gangrene, from great excess of stimulus. Thus, Regulus, after loosing both his eye-lids and his long confinement in a Carthaginian dungeon, upon sudden exposure to the rays of a meridian sun, must have been in the midst of darkness from the excess of surrounding light.

* De februm differentia, lib. i. cap. vii. chart. tom. 8. p. 115.

† Omnia hæc exinde tantum procedere, quod natura a primo morbi impetu quasi oppressa, devinctaq: non satis valida est ut symptomata regularia et magnitudini morbi consona exarserat; omnia vero phenomena prorsus sunt anomala. Etenim perturbata œconomia animali, et quasi disjecta, febris exinde deprimatur, quæ obtinente genuino naturæ ductu vigere solet. Syden. Schedul. Monit, de novæ febris ingressu, p. 541 et passim,

II. THE MODUS OPERANDI OF MERCURY, WHEN APPLIED TO THE SYSTEM.

I. As an evacuant of fæces, bile, mucus, and lymph.

To any one who considers the great degeneracy of some, and the increased quantity and vitiated quality of others of these substances in malignant fevers, the necessity of immediately discharging them will be sufficiently obvious; and accordingly, most prudent physicians have been anxious to excite stools as soon as the circumstances of each particular case would admit of, but generally with an intention of preventing an accumulation of putrid matter, and thereby obviating any farther accession to the putrid ferment, on which this state of fever is supposed to depend. In effecting this, from an apprehension of inducing a fatal debility, they have mostly confined themselves to the use of purges of the mildest nature, such as the neutral salts, senna, manna, cream of tartar, &c. But, since this supposed putrid state of fever has been proved to depend upon *excess* of stimulus, and since the application of a caustic § eating of a particular kind of fish †, and large doses of opium ‡ have each been known to produce the same alarming symptoms as are occasioned by what are generally called putrid contagions, this intention of cure proves to be badly founded, and is to be changed for the more rational one of *abstracting* from the sum total of stimuli. These matters are often so acrid as to excoriate the rectum and the skin of the external parts. When long retained they prove a nidus to infection, and concentrate it when already present. By mechanical pressure, from an accumulation of fæces in the large intestines, the capacity of the veins are diminished, the passage of the fluids through the smaller arteries is straitened, and a larger quantity of blood thrown on vital parts.

Both evacuations and dissections shew the encreased quantity and vitiated quality of the bile, which takes place in some of these fevers. Mr. Cleghorn mentions vast quantities of this fluid discharged in the malignant tertian of Minorca; and I have seen nearly two quarts evacuated in the short

§ Hippocrates' Epidemics, book iv,

† Desportes.

‡ Rush.

space of twenty-four hours. My own case in the bilious yellow fever of 1793, exhibited fully an equal quantity. It was so acrid as to excoriate the fauces, tongue, lips, and anus with the external skin wherever it touched. Doctor Physick's hand was inflamed by the acrid matter found in the gall, bladder, and primæ viæ in dissections made in this city, in the yellow fever of the same year. *The Doctor supposed this matter found in the intestines to be an altered secretion from the liver.* Mr. Cleghorn dissected nearly one hundred bodies that perished by the malignant tertian at Minorca, and says he always found the *vessica fellea* full and turgid and the stomach and intestines overflowing with bilious matter.

But, notwithstanding the large secretion of this fluid that takes place in some of these fevers, cases occasionally occur, in which it is *entirely absent*; in which the fæces first discharged are white and float on the surface of water like light wood, and in all other respects are similar to those accompanying the jaundice. Dr. Chisholm takes notice of their appearance in the fever of Grenada in 1792, and they frequently occur in the yellow fever, accompanied with great anxiety and soreness in the region of the liver; it is a symptom of dangerous prognosis, as it shews such an engorgement of the vessels of that organ as threatens an immediate paralysis or gangrene. This opinion is confirmed from the large quantities of bile and the relief afforded from the exhibition of purges which have a specific operation on the liver.

From the preceding observations and the dissections cited, it would seem that the matter found in the intestines was always merely a *vitiating secretion from the liver*; but from some experiments made on the black vomit discharged in the yellow fever of this city, in 1797, I am satisfied that *this is not* always the case, and that its sources are frequently *various*. In four instances I touched the tip of my tongue with some of this matter; in three of these cases there was some difference of appearance.

The first, at a small distance, very exactly exhibited the colour and consistence of coffee grounds; but, when more closely inspected, the fluid part was of a dark greenish colour, and the lumps brownish and soft. It imparted upon its first

application to the tongue, an intensely bitter and a very nauseous taste; in about half a minute a pricking sensation was perceived, which I can compare to nothing but that excited by the fine prickles of the prickly pear. The patient who discharged this matter, had vomited and purged bile *early* in the disease, and died on the fifth day from the attack, soon after I first saw her.

In the second case the discharge came on the seventh day of the disease, and at a small distance was similar to the former; it was found to be brownish, or rather of a chocolate colour, and extremely nauseous in scent and taste, but it did not impart the last sensation to the tongue. This patient had a hemorrhage from the gums, and the stools were similar to what was vomited up: she recovered after lying three days under this discharge without any perceptible pulse.

In the *third* case, the matter was discharged by a child about four years old, on the fifth day of the disease. She had *white stools* throughout the whole course of the disorder, and on the two first days nothing was discharged by vomit, but *mucus* and the liquids drank. She was early comatose, and discovered great anxiety on being roused. On the third day, the mucus became streaked, with a greyish colour, as if a small portion of ashes had been stirred in it, but on the fourth and fifth days it became very dark; although upon a *nicer* examination, it was still streaked with phlegm. This discovered, on its first application to the tongue, *neither taste nor smell*; but, in a short time excited the same pricking sensation as the first.

In both the first and third of these experiments, small pimples came out on my tongue, in a short time, and disappeared in about ten or twelve hours*. From these facts it appears,

* At the time of writing these observations on black vomit, I supposed this pricking sensation, and the pimples on my tongue, to have been occasioned by some peculiar properties of this fluid; but since, on repeating the same experiment, I have always found the *fatal* black matter insipid, and generally inodorous; except where it was acid, or occasionally mixed with putrid blood, (which last rarely happens). I conclude these effects must have originated in some cause which escaped observation.

1. That the black matter discharged from the stomach and intestines, in some of these fevers is often of different properties, and is to be referred to different sources.

2. That it is not always necessarily a fatal symptom ; and that when it takes place, the probable issue of the disease may be presaged from the knowledge of its source.

The first matter which I have noticed, from its greenish colour and bitterness of taste, may be considered as an altered secretion from the liver, with a mixture of blood from the ruptured vessels of that organ ; I refer its source to disorganization, and from this cause it must be necessarily a fatal symptom*.

The subject of the second experiment from its late occurrence, from the hemorrhage from the gums which attended, from its intolerable stench, from its want of acrimony, and more especially, from the frequent recoveries which take place after its occurrence, must have been grumous blood, issuing by diapedesis or rupture immediately into the stomach and intestines or swallowed from the mouth.

The last, from the gradual changes it went through in acquiring a dark colour, from its mixt heterogenous appearance, from the absence of a bitterness of taste, from its want of smell, and lastly from the discharge of white stools through the whole course of the disease, *I refer to an altered secretion from the arteries of the stomach, which, in a healthy state, were wont to secrete the mucus and the gastric fluid* †.

* Doctor Rush mentions two patients, which recovered from the yellow fever of 1793, after discharging black matter, on the first day of the disease, but as no mention is made of the lumps which give it what has been called the coffee ground aspect, and from his own remarks, I conclude it must have been nothing more than a dark bile, without any disorganization of the part from whence it was derived.

† ‘ The blood is here so much resolved, that before death it enters the smallest serous vessels, tinges the saliva and the serum discharged by a blister, and by oozing into the stomach gives that blackish cast to what is then thrown up.’ Pringle’s *Diseases of the army*, p. 197.

Sir John discovers much penetration in this remark, but, that he was entirely unacquainted with the *means*, by which this last discharge acquires ‘ that blackish cast,’ I shall take occasion to prove in another place.

Possibly cases may and do occur, in which all these fluids are present in the primæ viæ, at the same time ; but, from a determination of the fluids to any particular part, being known in some degree to exempt other parts from injury, such an occurrence must be rare. It may be said in contradiction to this remark, that in cases where the discharge indisputably proceeded from the liver, dissections have shewn the stomach and intestines in an inflamed or even in a gangrenous state ; but, this might be the effect of matter externally applied, which is known to destroy secretion, and from the immediate disorganization of the vessels, no discharge afterwards takes place until sloughs are cast off, which as coming from living and healthy vessels, must be always bland and inoffensive. This is illustrated from the effects of a caustic ; the part to which it is applied, first contracts and squeezes out the lymph before contained in its vessels ; a slough is then formed upon which all further discharge ceases, until that is cast off. The yellowness of the serum in these fevers has been supposed to depend upon bile in its *compound* state, but, experiment has convinced me that this supposition is also erroneous. In a case of yellow fever last autumn, I tasted the serum taken when the patient was very yellow ; it was unusually saline, but without the least bitterness of taste. A few days afterwards, I made the same experiment on the serum, from the blisters of an icteritious patient, which was the yellowest I ever saw, but not in the least bitter, nor unusually saline. The acrimony of the humours therefore, although the colouring matter of the bile is so eminently conspicuous, does not depend on the presence of that fluid in a compound state, but upon the acquisition of an increased proportion of salts.

I have been the more particular in this digression, not only with a view of elucidating the operation of mercury, in the cure of this state of fever, but, because the discharge of the black vomit, is so strongly associated with the death of the patient, as the inevitable consequence, that he is commonly deserted both by physicians and attendants upon its first ap-

pearance ; and often, when by a perseverance in proper remedies, life may be preserved. *

I now come to mention, in a summary view, the several good effects of mercury in evacuating the different humours, which have been noticed to be present in the first passages, in this state of fever.

1. By evacuating them early in the disease, any farther encrease of acrimony is prevented.

2. The generation or concentration of infection is prevented by the same means.

3. The stimulous from the mechanical pressure of hardened fæces is removed, the capacity of the veins of the larger intestines is encreased, and a revulsion from vital parts effected.

4. An accumulation of bile is prevented and its regurgitation into the stomach.

5. The white colour of the fæces is changed, the anxiety and oppression attending are relieved, and the yellowness of the skin prevented.

6. From its specific action on the hepatic system † the stimulus of contagion is superceded, congestion is removed, and hemorrhagy from that organ prevented.

7. In depleting the extremities of the vessels and affording an opportunity to them of contracting, it prevents hemorrhagy from the stomach and intestines.

* This took place in a case which I have mentioned in another part of this essay. A gentleman of respectability in his profession was called at the first appearance of this discharge, *in consultation with me*. Upon seeing the matter vomitted, he pronounced her irrecoverable, I told him, I supposed it consisted of blood, and was not the result of disorganization. He answered, that if I thought so, he should leave her in my care, as he did not think it worth while to take her case into consideration. He then went away without any other proposals. As the patient recovered, the truth of my suspicions was confirmed.

† For these effects of purging I refer to Sydenham ;
 Hillary on the diseases of Barbadoes ;
 Clark on diseases in voyages to hot climates ;
 And Doctor Rush's account of the effects of purging in the bilious yellow fever of 1793.

8. By creating an artificial weak part in the intestines, the effusion of lymph, serum, &c. in vital parts is prevented.

9. By removing acrid matter immediately from affected parts, it takes off indirect debility and strengthens the patient ‡

II. It induces a counter stimulus in every of the vascular system, and by a determination to the throat and mouth saves vital parts. The cure of all fevers, consists in exciting a new action in the vessels, or one different from that which constitutes the proximate cause of the disease ; * and accordingly no sooner do signs of the mercurial action appear than all untoward symptoms begin to decline. On its effects in dysentery, Dr. Clark has made the following remarks : ‘ For several years past,, says he ‘ when the dysentary has resisted the common modes of practice, I have administered mercury with the *greatest success*, and am thoroughly persuaded, that it is possessed of powers to remove inflammation and ulceration of the intestines in this distemper. † Dr. Gilchrist, at a much earlier period, has noticed this salutary property of mercury in curing inflammations. ‘ Nothing embarrasses more’ says the Doctor, than inflammation in a low state ; but, quicksilver is a powerful antiphlogistic, and removes inflammation without accelerating the motion of the fluids, which it rather diminishes by subduing their inflammatory disposition, when there is little or no fever, it as powerfully removes obstruction without diminishing the impetus of the blood ; on a proper degree of which resolution depends.’

I apprehend these effects in relieving intestinal inflammation may be referred to a threefold operation.

1. To its action immediately on the topical affection.
2. To its general operation on the blood-vessels.
3. To its operation in removing acrid matter from the parts affected by purging.

1. In relieving topical affection. This may be understood from its effects when topically applied to external ulcers, by which a sanious discharge is, in a short time, changed for

‡ In proof of this specific action, I refer to its effects, in what has been improperly termed Idiopathic hepatitis.

* Hence the action of Peruvian Bark, and other comparative low stimuli in curing slight cases of fevers, may be accounted for.

† Clark on diseases of voyages to hot climates.

a laudable pus. Before dismissing this subject; I must remark, that mercury appears to me, to possess exclusively the property of superseding the action of all animal poisons, whether generated in the body by altered secretions from its own organs, or derived from other animal matters externally applied. * This supposition derives support from its known effects, in the venereal disease, hydrophobia and small pox, which are all acknowledged to depend upon the specific stimulus of animal poison. Perhaps, upon this principle, it may be found a specific against the bites and stings of all venomous animals and insects:

2. Its general operation on the blood-vessels. For this purpose it must be introduced into the circulation. This may be done by the several modes to be hereafter described. Its action in this way is proved both from the change in the pulse and in the secretions succeeding its use, for an account of which I refer to authors who have employed it in the cure of febrile complaints:

3. By removing acrimony immediately from the parts affected. This effect has been before noticed in this treatise, when treating of its effects as a purge, to which I now refer.

The salutary effects of a spontaneous salivation, arising in the malignant state of fever, have been noticed by many writers on pestilential diseases. † But these effects are much more conspicuous from a mercurial salivation. I will here enumerate the most important.

1. When copious, by abstracting stimulus from the blood-vessels it gradually reduces the pulse and obviates the further necessity of purging and bleeding.

2. By producing a new excitement, and a different determination to the fluids, it relieves the burning of the stomach

* Dr. Rush in his account of the yellow fever of 1793, has remarked that swellings of the Lymphatic Glands did in no instance suppurate, where a mercurial salivation took place. Perhaps, this may be referred to this property of Mercury in counteracting or superceding the operation of the poison thrown on these glands.

† Sydenham's account of the continual fever of 1667, 68 and 69. Also, Huxham's Essay on Fevers.

and the distressing vomiting in these fevers. Large doses are more generally attended with this effect than small ones. This appeared in a very striking manner in the case published by me in October, 1797. The patient had been bled and purged freely, and was under the use of mercury, in nearly all the forms hereafter to be recommended, while the vomiting was still distressing, with little or no mitigation of the other symptoms, until by a mistake the nurse gave at one dose, a drachm of calomel which had been left to be rubbed on the lips and gums, after which, her vomiting suddenly ceased, and the other bad symptoms declined until the sixth day, when a discharge from the salivary glands appeared, which was succeeded by convalescence.

Dr. Rush, in his account of the yellow fever, of this city in 1793, says the 'effects of mercury in every case where salivation was induced were salutary.' Dr. Wade, in his account of the same disease in Bengal, gives the same favorable account of a mercurial salivation. Dr. Chisholm says 'that if salivation (in the Boullam fever) was speedily raised, the danger was removed, and the patient recovered,' and Mr. White, who practised in the same disease, declared, he did not loose a single patient, where a quantity of mercury had been given sufficient to excite salivation. I can also here add my own experience as testimony in favor of this evacuation. As in all the cases I knew or heard of, where salivation took place in the yellow fever of 1797, I know of but one where the disease proved fatal. This patient perished by a hæmorrhagy from the gums on the tenth day of the disorder, and after all other bad symptoms had ceased. I impute this unfortunate event to his not losing a sufficient quantity of blood early in the disease, as I did not see him before the fifth day of his illness. The advantages of salivation in malignant fevers are further established from its good effects in the plague at Algiers. One of the American captives, who was ransomed at the time when a peace was concluded between the United States and the Dey, and now in this city, asserts that mercury was in general use in the cure of that disorder, that he had it himself, and was cured by a salivation, with the assistance of sixteen bleedings.

There is also another advantage resulting from this action in the blood-vessels ; as relapses never take place where the discharge is sufficiently established *.

III. THE DIFFERENT MODES OF APPLYING IT, AND THE SEVERAL MEANS OF ASSISTING THE OPERATION.

And 1. By the mouth.

2. By the gums.

3. By frictions.

4. By shoes or socks impregnated with the ointment.

5. By ointment in the form of clysters.

6. By fumigation.

I. BY THE MOUTH.

Salivation may be induced by all the various and almost innumerable preparations of this mineral which chemistry presents ; but as it is necessary to keep up some purging during the whole course of these fevers, for the reasons before mentioned, and as calomel or the hydrargyrus muria-tus mitis possesses a purging property in an eminent degree over most others, and since it is one which has been most employed, and more especially since, from general use its virtues are better established, I shall prefer it to every other preparation for internal use, in this state of fever. In the first days of these fevers, from the observations before made, strong purges are indispensable, and from their rapid progress and dangerous tendency these are to be so employed, as to produce as speedy an operation as possible. For this purpose, ten or fifteen grains of jalap are to be combined with ten grains of calomel, and given every three hours until a discharge is procured. As pills are known, sometimes to pass through the whole tract of the intestines in an undissolved state, and from the obstinate obstruction which here attends, a large quantity of these purges may, probably, if given in the form of pills, be accumulated in a portion of the intestines and become productive of inflammation and its consequences ; they are always to be given in the form of powders. I know to those who are

* Vid. Chisholm on the Malignant Pestilential Fever, &c.

regulated more by the names of diseases than their symptoms, that such doses may seem inadmissible; but experience has established their safety, and we are here 'to attend more to the effects produced than to the dose*.

In many cases five or six of these doses are necessary before the desired effect is produced. Mr. Wm. Bunting's apprentice boy, who was but eleven years old, in the yellow fever last season, took two doses of five grains of jalap, with the same quantity of calomel, each at the interval of two hours, and afterwards, four doses of ten grains each, at the interval of three hours between, before any evacuation took place. To induce salivation, from three to five grains may be given every three hours, and cases occur, where ten or even twenty grains may be given at the same intervals of time, not only with safety, but with advantage. Doctor Chisholm gave nineteen grains three times a day, and the patient became convalescent after the tenth day, when he had taken two hundred and fifty-four grains. I have even gone farther in its internal use with the greatest success. In one case, which I have before cited, the patient took three hundred and fifty-six grains internally, in six days, during which time, every other method of introducing mercury into the system was employed.

At seeing this account let not the rigid dogmatist contract his brow into a frown of disapprobation; but, rather let him learn, that where we would meet with success 'we must always accommodate the curative force to the morbid, or to the cause of the disease*.' The form of powders is here also recommended in preference to pills or bolusses; both for reasons before mentioned, and because, in swallowing them, part of the calomel adheres to the fauces and throat, by which means, the absorbing surface is increased. If the calomel be triturated with twice its quantity of loaf sugar, the patient, before an advanced stage of the disease, can take it without any other addition; but, in this case, from the dryness of the mouth, fauces and œsophagus, and from the debility of

* Pringle's Diseases of the Army.

* Brown's Elements.

the organs of deglutition, a small portion of some liquid must be added to facilitate its passage into the stomach.

In this way a salivation is often excited in slight cases, in the space of thirty-six hours ; but, in cases of more violence, a perseverance for five and even ten days is oftener necessary ; and sometimes the irritability of the intestines is so great as to render it impossible to excite this discharge by any internal preparation whatever. When this happens, they are to be laid aside, and recourse is to be had to some, or all of the means hereafter described.

2. By the internal surface of the cheeks and lips and by the gums.

Mr. Clare particularly recommends this mode of introducing mercury into the constitution *, and Doctor Woodhouse found it speedy and effectual in the yellow fever of 1793. I suspect it was effectual only when received into the circulation ; and as it is probable salivation may be induced merely from the local and partial operation of mercury on the salivary glands, without its producing any change of action in the general system of blood-vessels, its use is not to be relied on, without employing other means at the same time †. To introduce mercury in this way, calomel is rubbed in on the gums, and the inside of the lips and cheeks, or the mouth is washed with a solution of corrosive sublimate in water several times in the day.

3. By mercurial frictions.

The great number of absorbents, spread over almost the whole surface of the body, and the effects speedily produced by mercurial frictions, eminently prove the facility with which

* A New Method of introducing Mercury into the circulation, by P. Clare, Surgeon.

† The late Doctor William Annan, in his last illness in the Yellow Fever of this city in 1797, exhibited a melancholy proof of the local effects from this mode of applying mercury. He had often during his illness, washed his mouth with a solution of corrosive sublimate, and when I saw him, two days before his death, and insisted on his using more mercury internally, he urged as an objection, that his mouth was already sufficiently affected by the use of this gargle.

in this way enters the constitution; and the determination which it discovers to pass off by the salivary glands, when thus introduced, renders it advisable to make an early trial of its efficacy in all cases of great danger. To produce as speedy an effect as possible, the whole body and extremities are rubbed twice or thrice a day, with large quantities of the strongest mercurial ointment. In more ordinary cases, rubbing the upper and lower extremities, particularly in the course of the absorbents, with half an ounce of the same ointment twice a day, until it nearly disappears will be generally sufficient. Before each inunction, it is necessary to wash the parts to which it is applied with soap and warm water: as by that means the mouths of the absorbents, freed from the oily matter left from the former applications, come more immediately in contact with the fresh ointment. In these frictions, the nurse or person who performs should take the precaution of inclosing her hands in bladders, otherwise, from the great facility with which it enters healthy vessels, to that with which it enters those under morbid excitement, she will be salivated before any effect is produced on the patient.

4. By impregnating shoes or socks with mercurial ointment.

This method, as far as I know, was first introduced into practice by me in the last fever that infested this city: and notwithstanding it has been supposed, that mercury enters the absorbents with great difficulty when applied to the skin without friction, experience has proved, that it is easily taken up when applied in this way to the feet. At the time when I first began to use this mode, the necessity of employing other means in the same cases, where these socks or shoes were worn, rendered it impossible to ascertain the time required to affect the mouth by this way, but since I have salivated a venereal patient in four days time, by their use alone. In this mode shoes or socks prepared of bladders are to be well lined with strong mercurial ointment, and confined on the patients feet.

5. Mercury has also been employed, with supposed advantage, in this state of fever, by clysters, prepared by adding

the common ointment to starch, or oatmeal gruel. These are to be frequently injected from time to time.

Probably Plenck's solution would here answer a better purpose ; as the mercury in this preparation is less clogged, and from the absence of oily matter, would be less likely to be immediately returned.

6. By fumugations,

This mode of affecting the system, is remarkably speedy, in so much that the mouth is sometimes affected in the short space of ten or twelve hours ; but much danger has been apprehended from the application of these vapours to the lungs. This may be prevented by confining them to particular parts and in dangerous cases, we are by no means to lose the probable advantages to be derived from so powerful a remedy, on account of such futile objections. It is applied by sprinkling cinnabar of mercury, on coals contained in some convenient vessel for the purpose, and holding it under the hams of the patient, while he lays on his back, with the knees in an inflected posture. The bed cloaths are to be well confined to prevent the escape of fumes, and their affecting his lungs.

From the aptitude of these fumes to affect the bowels with gripes and to pass off by stool, they ought always to be applied in a small quantity at a time, and frequently repeated ; but they are immediately to be laid aside, as soon as the least affection of the bowels is threatened.

In whatever way mercury is employed in the beginning of these fevers, and more especially, where much febrile heat prevails, its operation is particularly assisted by blood-letting and the application of cold water, cool air, and even ice to the skin.

This practice has unfortunately been the subject of much obloquy amongst those physicians who are riveted to the dogmas of the former theory of the action of mercury ; but as this theory is proved to be erroneous, and is now on the eve of everlasting oblivion, the fabric will ere long share the same fate with its tottering foundation.

So far are cold applications from injuring the constitution, their effects prove them always salutary ; as they become an evacuant by abstracting heat ; they relieve that intolerable

burning of the skin, which is so distressing in this stage of fever; and so far are they from militating against the action of mercury, that by lessening the prevailing febrile action they powerfully assist it. Nor is this city the only place, where the salutary effects of these applications have proved so conspicuous, as to warrant their propriety and general safety. A Dr. Armstrong in the West Indies, with a view of assisting the operation of mercury in the cure of malignant fevers, is said to wash his patients frequently in cold water, with the greatest success.

But in more advanced stages of the disease, where the skin is cold, and the pulse feeble or imperceptible, every stimulating application to the surface favours our views. For this purpose are to be applied sinapisms and blisters to the extremities; and cloths wrung out of hot brandy, saturated with sea salt, are to be successively laid over every part of the body, and renewed as often as they become cool.* Hot bricks or jugs filled with hot water are also here applied with advantage.

When we have produced the desired effect, from the use of one or all these modes combined, it sometimes happens, that the profuse discharge from the salivary glands, and the intolerable pains in the jaws and teeth, become objects of attention. When this occurs three indications present.

1. It is necessary to mitigate pain.
2. To moderate the discharge.
3. And heal the local injury.

The first intention, as we are, from the nature of the disease which preceded, and from apprehensions of relapse, debarred from the internal use of opium, is answered by topical applications. For this purpose opium is dissolved in water and the mouth washed frequently therewith, but I have lately found from experience that milk answers the intention much better than simple water. My first inducement to use it, was

* Fomentations of these substances were first employed by me in the endemic of this city in the year 1797. I know of no external application more powerful in exciting the heat of the skin; even epispastics not excepted.

from its known properties of obtunding acrimony. My method is to triturate half a drachm of opium, in an ounce of pump water, and, when sufficiently dissolved, to add four ounces of new milk; the mixture is then to be used as a gargle as often as the pains, &c. render it necessary*.

The second indication is answered, by determining the fluids to other parts. The fluids are called off by purges or laxatives, and blisters.

Sulphur has been particularly recommended by some, not only as a laxative, but as it is known, when combined with mercury, to render it inert, it has been supposed to form such a combination in the system, and thereby to possess advantages peculiar to itself; while others have denied it to possess that property. Which of these opinions is most correct, I will not venture here to decide; but as the flowers of sulphur prove an efficacious and convenient laxative, and as they are possessed of equal virtues (if we do not allow them any superiority) they are still preferred. As we are here to keep up a gentle and regular discharge from the intestines, small and frequent doses are recommended.

When the swelling of the salivary glands is great, blisters are to be applied, either immediately to the swellings, or on the back of the neck.

3. The local injury is healed by increasing the tone of the parts affected.

The tone of the parts is increased by astringent gargles, composed of red rose leaves, red oak bark, or galls, with a small portion of allum and honey. Mr. Bell recommends a strong solution of borax, as possessed of peculiar virtues in answering this intention; but, from what trials I have made of this, I am inclined to think it inferior in efficacy to several of the astringents before mentioned.

The introduction of mercury into more general practice, has been retarded from a supposition of its injuring the teeth and constitution; but these effects are imaginary and without

* Since writing this dissertation, I have found a *very strong* infusion of the finer teas (*thea viridis* linn.) to answer the purpose still better.

foundation; as, where the teeth have been sound before its use, and the patient has been *diligent in cleansing* the mouth, while under salivation, they have never been known in a single instance, to sustain any injury †. To refute the supposition of injury done the constitution, we need only refer to the constitutions of those, who have recovered from malignant fevers by its use. Many have been cured of obstinate ulcers, swellings of the joints, rheumatic pains and chronic obstructions of the viscera, and most enjoy better health than they ever had before its use. But, grant for a moment that the constitution be injured, and the teeth destroyed from the use of this remedy. Can the loss of a tooth or a trifling injury to the constitution be put in competition with almost certain death? I say almost certain, as the chance of recovery in malignant fevers, without its use, is, at most, as one is to two. And I am firmly persuaded that, by its free and proper use, with the attention of nurses, and a strict adherence to the anti-phlogistic regimen in the beginning of the disease, ninety-nine cases of an hundred will terminate favorably. The belief, therefore, 'that the yellow fever, plague, or other malignant diseases are necessarily mortal, *will be proved* to be as much the effect of a superstitious torpor in the understanding, as the ancient belief that the epilepsy was a supernatural disease, and that it was an offence against heaven to attempt to cure it ‡.'

† Vid. Rush's works.

‡ Dr. Rush on the Bilious Yellow Fever.

AN INAUGURAL
EXPERIMENTAL ENQUIRY,

BEING

AN ATTEMPT TO PROVE,

THAT

DIGESTION IN MAN,
DEPENDS ON THE UNITED CAUSES

OF

SOLUTION AND FERMENTATION:

SUBMITTED TO THE EXAMINATION OF THE

REVEREND JOHN EWING, S. T. P. PROVOST;

THE TRUSTEES AND MEDICAL FACULTY OF THE UNIVERSITY
OF PENNSYLVANIA,

FOR THE DEGREE OF DOCTOR OF MEDICINE;

ON THE THIRTY-FIRST DAY OF MAY, A. D. ONE THOUSAND
EIGHT HUNDRED.

BY JOSEPH GLOVER, OF CHARLESTON, S. C.

MEMBER OF THE PHILADELPHIA MEDICAL AND CHEMICAL
SOCIETIES.

*Tentanda via est, quàm me quoq' possim
Tollere humo.* *Vir.*

PREFACE.

THE difficulty of experimenting, appears to be the reason why physiology has not kept pace with other branches of medical science. In most of these we may travel on a smooth and delightful road ; but the most trivial circumstances influence the result of experiments ; thus assertion is opposed to assertion, and on the reputation of the author rests the position.

In the following pages it is contemplated to investigate the process of digestion ; a subject by no means perfectly understood. In doing this, I have endeavoured to arrange such ideas on the subject, as I have collected from the opinions of others, or from such facts and experiments as I have myself ascertained. These have taught me the difficulty of explaining the phenomena of digestion on most of the theories which have been advanced ; nor do I expect that the one which I have adopted, is void of imperfections. Too fond of reducing every operation of the animal economy to a single principle, many physiologists have explained the process of digestion on some favorite theory, and thus, by setting a limit to the hand of nature, have left unaccounted for some of the most important phenomena.

The result of my investigations, such as it is, circumstances have induced me to cast as my portion into the scale of science. Should it suggest an idea worthy the attention of the philosopher, I shall feel highly gratified ; but, on the contrary, should my experiments prove inconclusive, and error stamp the reputation of my essay, still I shall be pleased with the reflection, that the most feeble attempt to elucidate this

important branch of physiology, can be by no means injurious to science.

The haste, so unavoidably annexed to the short time allotted for this publication, will no doubt induce the reader to overlook the many inaccuracies of language, which I am fully aware are contained in the following pages. They may perhaps bring to his recollection, that sentence of Doctor Beddoes so expressive of the liberality of the philosopher, when he says, 'we should set a due value on our present knowledge, though it be imperfect; and restrain those rude hands, that are ever ready to pluck up the tender plants of science, because they do not bear ripe fruit at a season when they can be only putting forth their blossoms.' Impressed with the generosity of these ideas, I submit my observations and experiments to the candid investigation of the philosopher, whose patronage alone stamps the merit of every youthful performance, gives activity to the mind, and sanctions future investigations.

EXPERIMENTAL INQUIRY.

SECTION I.

OBSERVATIONS ON DIGESTION.

MAN, like every other being in nature, requires a continual and regular supply of food, for the several purposes of supporting life, of promoting the growth of his system until he arrives at maturity, and of forming new parts when such become necessary. Indeed, on contemplating the complicated structure of his frame, it is evident, that a continual loss of the solid and fluid parts, of which he is composed, must be the inevitable effect of every action or function of life which he performs.

Nature, always wise, to obviate this general waste of his system and continual tendency to decay, made it necessary that he should have some inducements to repair it, and thus be reminded of the connection which exists between aliment and life. Accordingly we find he is indued with the stimulus of hunger and thirst, which, together with the pleasure he receives from gratifying those appetites, induce him to take into his stomach a certain quantity of matter to allay those disagreeable sensations. This matter includes not only the several kinds of animal and vegetable substances which we denominate food, but also comprehends the fluids taken in with which they are diluted.

These having arrived at the stomach which is the great receptacle of his aliment, a greater or less length of time is requisite, according, to circumstances, for them to undergo

those processes which are essential to their assimilation being completed. In fact, I conceive it an impossibility to determine with any certainty, the exact time necessary for the digestive organs to perform their respective functions as that will depend in a great measure on the strength of those organs, on the quality, quantity and manner in which the food is prepared, its previous mastication, and various other causes of which we are not always aware. The most prevalent opinion on this subject, is, that from about the third to the sixth hour after food is taken, it is discharged, through the pylorus, of a pultaceous consistence. There are however some extraordinary deviations from this allotted time, which cannot but convince us of the great uncertainty of calculations of this nature. We have on record instances related of substances remaining in the stomach indigested for months; and on the contrary, that in two hours after food was taken into the stomach, that organ was found empty.* These I conceive to be rare occurrences, neither do I believe it by any means common, even in the space of three hours, for the stomach to discharge its contents; as in a majority of mankind, I presume, a much longer time is requisite for food to undergo those changes, which are usually effected on it during its stay in that organ.

To our several kinds of aliment, different condiments are added by various nations, many of which merely gratify the palate, while others assist in promoting digestion. Among us at present sea salt is most universally used for this purpose.† Professor Chaptall tells us, that the acidulous tartrate of potash is greatly consumed in the north of Europe, where it is used as a table seasoner; and Professor Barton has informed me, that the Creek Indians make use of hickory ashes, and that in some of the southern states, the ashes of a particular kind of marsh-grass were formerly preferred for the same purpose.

* Vid Haller's Element. Physiol. Tom. VI. page 281.

† According to the experiments of the celebrated Pringle, a small quantity of sea salt hastens putrefaction, while a larger quantity retards that process.—Diseases of the army, Appendix; paper 3. Exper. 25.

Besides this difference in nations with respect to condiment there is one of still more consequence, which is, their striking peculiarities in the choice of food. We find, that in Lapland, Iceland, Greenland, Norway, and other cold countries, the inhabitants live chiefly on fish and flesh;* while on the contrary, we are informed of certain sects in India, who live almost, if not solely, on a vegetable diet. Both the Laplander and the Indian enjoy their health in these extremes; their habits, together with their climates, being better adapted to their respective modes of living; as the southern latitude in which the latter resides, appears to prevent his subsisting for any length of time, on fish or flesh; while its stimulus is absolutely necessary to support the general waste of the system, to which the former is exposed from cold. A majority of mankind live in the medium of these extremes; experience having taught them, that a due proportion of animal and vegetable food, is the better adapted for their nourishment, the one counteracting the ill effects arising from the other.

Of all animals, man appears to be the most omnivorous. Destined to range through every the most distant part of our globe, he is capable of accommodating himself to the food of every country. Certain other animals are likewise capable not only of changing their accustomed diet, but sometimes acquire so vitiated a taste, as to refuse their former food. This was particularly the case with the wood pigeon of Spalanzani. This acute physiologist tells us, that by dint of hunger he brought this bird to relish flesh so well, that it refused every other kind of sustenance, even grain, of which, it is naturally so greedy. Various other facts of a similar nature, are found on record. Von Troil informs us, that the Icelanders, when there is a scarcity of fodder, feed their cattle with *steenbitr*, (a kind of fish) which, together with the heads and bones of cod, is beaten small, and mixed with one quarter of

* This is sometimes though more rarely, the case in southern latitudes. At Orange river, in Africa, Fordyce tells us, that the inhabitants live upon limpets, dead and putrid seals and whales, not tasting a particle of vegetable food of any kind, excepting aromatics.

chopped hay. He further adds, that the cattle are fond of this food, and yield a good deal of milk after having used it.*

Professor Barton, in his course on natural history, has likewise related a fact no less interesting. He has told us, that deer have been frequently observed to feed on dead fish, which had been washed up on the banks of the Susquehanna and other rivers. These are instances sufficient for my purpose, but many others of equal importance might be collected.

From the several kinds of aliment taken into the stomach, man is plentifully supplied with fluids, and from the component parts of these fluids, is the growth of his system and the solids of his body produced. This growth of his system and production of solids is induced, although he may subsist on very different kinds of food, as by the peculiar operation of his digestive organs, he is capable of assimilating, by certain processes, matters taken from either the animal or vegetable kingdoms into a fluid *sui generis*. These processes of assimilation are comprehended in the term digestion; by it we are to understand, those processes which take place in the digestive organs of man, and by which his food is converted into laudable chyle.

The *modus operandi* of nature in this conversion of our food into chyle, has attracted the attention of philosophers in every age, and various theories have been advanced to explain the phenomena which occur. With this view we find, that, the theories of the *heat of the stomach*, of *mechanical action*, of *fermentation*, of *solution* and others, have all been advocated by men, whose fame has added reputation to their opinions. But, as no one of these can exclusively account for all the phenomena of digestion, and as in the choice of a theory, the preference should always be given to such a one, as will explain to us the most of them; I am induced to adopt another, and attempt to prove the dependency of this important function, on the united causes of solution and fermentation.

* Vid. Letters on Iceland. p. 133.

OF THE HEAT OF THE STOMACH.

The theory of the heat of the stomach, was at one time so fashionable, that Professor Blumenbach tells us, instead of the term digestion, that of coction, was formerly used by the greater part of physiologists *. This opinion, however, I believe at present has but few advocates, as I presume no person will now contend that heat is the sole cause of digestion. This would be equally as incorrect as to say, that it does not assist in promoting that process. While we refuse to admit that heat is the sole efficient cause of digestion, we cannot but acknowledge its effect in expediting that process, as it has been long since made evident by experiment. It therefore only remains that we should shew from the situation of the stomach, that it is advantageously seated to be supplied with heat from its neighbouring parts, as we may easily conceive from contemplating their relative situations with respect to it. We find that its right side is covered by the thin edge of the liver; its left touches the spleen; that behind it is the seat of the pancreas, and immediately above it is the diaphragm; that the peritoneum lies before it, which, by the action of the abdominal muscles, gives it a motion diametrically opposite to that which it receives from the diaphragm in respiration; and that the aorta, the largest artery in the body, lies just behind it. All of these circumstances must tend to give it additional heat. Hence we may with propriety acknowledge the accuracy of Doctor Barry, when he tells us, that ‘the heat of the stomach in a healthy man, is greater than the common heat of sun in a summer’s day †.’

OF MECHANICAL ACTION.

To disprove that the mechanical action of the stomach has any effect in promoting digestion, I need say but little. Facts speak for themselves. That accurate experimenter, the Abbe Spallanzani, has decidedly proven the very wonderful muscular action exerted in the stomachs of some animals; but his

* Institut. Physiol. vol. II. p. 23.

† Vid. Treat. on Digest. p. 8.

experiments likewise tend to shew, that no such action takes place in the human stomach. Having frequently swallowed wooden tubes during his experiments, which were made so thin as to be incapable of bearing the slightest pressure, he never, in a single instance, discovered one of them to be broken. In addition to this, he mentions the fact of cherries and grapes being voided entire, as I have myself frequently observed. He likewise relates an experiment with ripe grapes, which we all know to be incapable of bearing the least mechanical action, which appears to be directly in point. ‘Of twenty-five,’ says he, ‘which I swallowed, eighteen were voided entire, of the other seven, the skins only appeared †.’ These experiments alone, I deem sufficient to prove, that no triturating power is possessed by the human stomach, particularly as the fact of grapes being voided entire, must be notorious to every person who has attended to the subject. In short, I do believe, that the muscular fibres of the stomach, have no other effect on our food, than merely that of propelling it through the pylorus.

OF FERMENTATION.

While some physiologists of considerable reputation, have considered fermentation as quite sufficient to explain all the various phenomena of digestion, others of equal celebrity have contended, that no such process takes place in a healthy stomach. This diversity of opinion, I cannot attribute to motives of prepossession in favor of any particular theory, but would rather presume, it was the consequence of a supposition that to admit the one to be a fact, would be a tacit acknowledgment that the other could not be true. This too I suppose is the reason why even at the present time, those who have ascertained the solvent power of the gastric juice, will not admit that fermentation ever takes place in a healthy digestion. But this perhaps though too common an error, is still one by no means the less prominent. Does not chemistry teach us, that nature frequently requires in her operations a multiplicity of causes to induce a single effect? Why then,

† Natural History, vol. I. p. 222.

if one of the causes, which have been advanced, is not sufficient to explain the phenomena which occur, should we attempt the establishment of another, equally inadequate to account for the wonderful effect of the conversion of our food into chyle?

The operations of nature are uniform, and frequently too deep for the shallow limits of human wisdom to demonstrate ; but I think, when we shall have been more successful in our experiments, it will be found, that digestion depends on the combination of several causes, and that fermentation does certainly take place, as I shall endeavour to prove in a subsequent part of this essay.

OF SOLUTION.

That solution does likewise take place in digestion, I do believe, as the experiments of many physiologists of considerable reputation tend to prove the position, and my own have induced me to embrace it. The opinion, however, is by no means novel. Barry informs us, that ‘ Basil Valentine was the first among the chemists who supposed that animal digestion was owing to an acid dissolving menstruum.’ That ‘ Paracelsus received this opinion from him.’ And that ‘ Van Helmont carried it farther ; and asserted, that the spleen prepared this menstruum, which was from thence conveyed into the stomach, by the *vasa brevia*.’ Hence it appears, that the theory of solution has been long since favoured. Since when, very accurate experimenters have written, in hopes completely to decide the question in its favor. But, although they have most demonstratively proven the solvent power of the gastric juice, they have by no means shewn that fermentation does not likewise take place. For my own part, I do not hesitate in believing, that both solution and fermentation do take place in a healthy digestion ; indeed I think, with correctness I may venture to assert, that in the human stomach, fermentation does as necessarily follow solution in the conversion of food into chyle, as thought succeeds impression in the formation of ideas.

Food, in the first instance, is considerably attenuated, by the mastication which the rotatory motion of our jaws and pressure between our molares are so capable of giving it. Its particles being thus divided, are intimately mixed with the mucus of the mouth and saliva, after which it passes down into the stomach. This we may term a process preparatory to digestion. It is certainly one of much more consequence to the perfect digestion of our aliment, than is generally supposed; as it is evident those persons, who half chew or bolt their victuals, as it is called, are generally subject to all the numerous diseases arising from indigestion. Hence appears the necessity of persons being particular in the mastication of their food, as nothing scarcely can be of more injury to the constitution, than continually to supply the stomach with indigestible half-masticated food.

Something, in its effect very similar to mastication, is observable in domestic fowls. They by a peculiar instinct, take into their gizzards, pebbles and gravel, which certainly serves in them every purpose, which teeth do in some other animals. I have made use of the word instinct through choice, because I cannot believe with Spallanzani that they are picked up by mere accident, or through their ignorance in mistaking them for food. One fact appears to oppose his theory, which is, that those fowls which are kept on a gravelly soil are rarely if ever found to have a greater number of stones in their gizzards, than those raised where less gravel is present. Again, if they were picked up by accident, we should expect that they are not at all necessary to their health; whereas, the very reverse of this is the fact. A very respectable author, who investigated this subject with considerable success, by experimenting on chickens hatched with artificial heat, has given us the very best information I have perused. He says, 'I have hatched vast numbers, and frequently have given the chickens small seeds whole, taking care that they should have no stones. In this case the seed was hardly digested, and many of the chickens died. With the same treatment in every respect, others who had their seeds ground, or have been allowed to pick up stones, have none of them been

lost.* This would appear to shew, that pebbles are essentially requisite to the healthy digestion of these animals. Indeed, the experience of many persons tends to prove this to be the case, as we often hear of their sending for gravel for their poultry, and when interrogated why they do this, they tell us that without it their fowls grow poor, dwindle away, and sometimes die. Mr. John Hunter, commenting on this assertion of Spallanzani, that pebbles are picked up by birds through chance or ignorance, says with much humour that 'it appears singular, that only those which have gizzards should be so stupid.'

The more freely food is masticated, and the more minutely it is divided; the less heavy does it lay on the stomach, and the more easy it is of digestion. The experiments of Spallanzani,* made on himself, prove the latter position; the former is obvious to common observation. Mastication not only promotes digestion, by minutely dividing the matters to be carried into the stomach; but likewise, by mixing them with saliva to form a pulpy mass, it involves a very considerable portion of atmospherical air, which, being rarefied by the heat of that organ, tends considerably to burst the several particles of food from each other. Here then we see that it indirectly promotes solution, for, as the several particles of food are separated from each other, in that proportion will solution be expedited; because, a greater number of surfaces will thereby be exposed to the action of the gastric juice, and of consequence the food will be the more speedily dissolved.

In the investigation of a subject like the present, it is necessary that we attend particularly to the causes which influence digestion, and judge of their effects, by comparing them to the effects of similar causes out of the body. And again, by accurately examining the products of digestion, and by comparing them to similar products out of the body, thereby investigate its causes. Thus, as all vegetable substances, capable of undergoing the vinous fermentation, contain the constituent principles of carbonic acid and of alcohol, and as these

* Fordyce on food, p. 24.

† Natural History, Vol. 1. p. 224.

substances are obtained from such vegetables, the former during the fermentative process, and the latter after that process has ceased; it is evident, if such products are obtained from the digestion of food in the stomach, it must be the effect of a similar process. The same observations will apply to the other stages of fermentation.

In the following pages, I shall endeavour to relate in brief detail, such facts and experiments as have induced me to adopt the present theory. This will be done in three several sections. In the first I shall proceed to deliver a few observations on the matters which influence digestion. The second will contain an attempt to shew, how far solution is admissible in digestion. And in the third and last, I shall endeavour to relate, why it is presumable that fermentation does likewise take place.

SECTION II.

OF THE MATTERS WHICH INFLUENCE DIGESTION.

THE chief of these are, saliva, the gastric juice, the mucus of the primæ viæ, the bile, and the pancreatic juice. And,

1st. OF THE SALIVA.

By saliva we mean that colourless, glutinous, and resolvent fluid, which is secreted by the parotid, submaxillary, and sublingual glands. It is perfectly tasteless and inodorous in health, and consists of water holding a mucilage and certain salts in solution.

On being placed on the fire, its watery part speedily evaporates, leaving its salts in a state of crystallization, resembling two or three different kinds of salt. These are, according to Fordyce, sea-salt and sal ammoniac, together with various other crystallizations of irregular forms; and agreeable to Plénck, sal ammoniac and animal earth; the former he inferred from triturating quicklime with saliva; the latter from salival calculus and the products of fire.

Saliva from its sapid nature, gives an increased flavour to food. It tends to relieve thirst, by supplying the mouth and

fauces with a sufficient quantity of moisture ; and has a certain and evident effect in digestion. It does not however, possess a solvent power, as has been asserted by some, at least it does not possess this property in a greater degree than simple water.

I put an ounce of pure saliva, and as much of simple water, into two separate phials ; to each of these I added two drachms of roasted veal. These phials were placed uncorked in a sand bath, which was kept as near as possible to the heat of the human body. After suffering them to remain at rest for eight and forty hours, I decanted the water and saliva from each of the phials. The veal which had been immersed in water, and that which had been in saliva, were now placed separately on bibulous paper, and as much of their moisture removed as possible. The one which had been in water, was now weighed ; it had lost twenty-three grains of its weight. On weighing the other, it had lost no more. This would certainly seem to shew, that saliva has no solvent power, at least not out of the body.

Again, it has been asserted, that saliva corrodes copper and iron more speedily than simple water *. To ascertain this point, I made the following experiment :

Having placed twelve grains of sheet copper in a phial containing saliva, and the same quantity in another of equal size, containing water only ; I put them both in a sand bath, which was generally about the temperature of one hundred of Fahrenheit. They were both kept in this situation for one week, at the expiration of which time, they were taken out, wiped dry, and weighed. I could not discover, that either one of them had lost more of its weight than the other. From this I presume we may conclude, that saliva has not a much greater effect, if any, in the corrosion of copper, than simple water.

Desirous of knowing what effect saliva has in digestion, and with a view of ascertaining whether it possessed a fermentative property or not, I exposed equal portions of leavened

* Vid. Haller's Element. Physiql. Tom. VI. p. 54. and Plenck on Hum. fluids, p. 72.

bread and roasted veal, in two separate flasks, to the heat of a sand bath, which I was careful to keep as near as possible to the temperature of the human body, or from ninety-six to ninety-eight of Fahrenheit. To one of these flasks, I had previously added eight ounces of water; to the other, seven ounces of water and one of saliva. The veal, to which saliva was added, I took the precaution of masticating, in order that it might be the more accurately blended with that fluid. In the space of six hours, the one containing saliva smelled a little sour, emitted air bubbles, and shewed evident marks of fermentation. Whereas that process did not commence in the other, which contained water alone, until four hours after.

Professor Rush has long since made a similar and very decided experiment. ‘To elucidate the properties of saliva,’ says our author, ‘I placed mutton and bread, of each two drachms, in two glass vessels. To one I added an ounce and a half of saliva, to the other the same quantity of water, and placed both of them in a sand bath. Five hours having elapsed, the mixture with saliva began to ferment. In seven hours it discovered evident marks of acidity; whilst, in that, to which water was added, scarcely any motion was perceptible. After twelve hours had elapsed, the mixture with saliva emitted a putrid smell; whilst the mixture with water remained mild and inodorous to the twentieth hour*.’ An experiment somewhat similar, has likewise been made by Pringle, but not with exactly the same success. He exposed two drachms of fresh meat and the same quantity of bread, together with water and saliva, to the heat of a furnace, kept at the temperature of 100 of Fahrenheit. The mixture remained about two days, he says, with scarce any visible fermentation; but on the third day that process became manifest†. This investigation, however, does not appear to have been made sufficiently fair, for he tells us, that his experiment was made in a closed phial. Now we know that the presence of vital air, is a circumstance essential to fermentation, and hence its exclusion must have been the cause of that process being retarded. In every com-

* Inaug. Diss. de Coctio. Cib. in Vent. Exper. V.

† Vid. Diseases of the Army, Appendix, paper 4, Exper. 30.

parative experiment like this, every circumstance should surely be made as similiar as possible, to those which occur in the living stomach, and of consequence, there should be an admission of vital air, as this fluid is always involved in saliva, which is several times in the course of a single minute conveyed into the stomach.

We are informed, that some uncivilized nations, are so well aware of the fermentative property of saliva, that they prepare an intoxicating drink, by mixing saliva with certain vegetable substances. Plenck relates that they prepare it from the chewed roots of the *jatropha manihot* or *cassada* and *piper methisticum*.

Whether saliva possesses a septic or antiseptic quality, has likewise been a subject of some controversy. To satisfy myself on this head, I placed equal quantities of roasted veal, in two separate phials of the same size. To one, I added an ounce of pure saliva, to the other, as much of simple water; these I placed, uncorked, in a sand bath, which I endeavoured to keep as near as possible to ninety-six of Fahrenheit. The heat of the bath, however, was sometimes as high as one hundred and ten, but never below fifty. In twenty-two hours, the one containing saliva emitted a putrid smell; the other only smelled sour. In eighteen hours after, I examined the phials again; they both smelled putrid; the one with saliva was the most so, and likewise had changed its colour a little, which was not the case with the other.

From these experiments, I presume it appears, that saliva possesses the property of promoting both fermentation and putrefaction; and not that it promotes fermentation, and at the same time retards putrefaction, as has been supposed.

The quantity of twelve ounces of saliva, is generally supposed to be secreted by an healthy adult, in the space of twenty-four hours. At least this is according to the experiment of Nuck, whose assertion I believe continues to be considered as the most correct. The quantity however, is capable of being wonderfully increased by certain medicines and other stimulants. Indeed Haller speaks of fifteen ounces being effused in the space of thirty minutes*.

* Vid. Element. Phisiolog. Tom. VI. p. 59.

2d. OF THE GASTRIC JUICE.

The gastric juice in health, is a colourless and turbid fluid, void of both taste and smell; and I must add, bearing considerable resemblance to saliva. I do not believe, as some authors have supposed, that it is secreted from the small glands of the stomach, as it is inconceivable to me, how such minute glands could be the source of such large quantities of this fluid, as are at times secreted. I therefore shall prefer embracing the theory of those, who suppose the immediate source of the gastric juice to be the extremities of the arteries of the stomach; for I can as easily conceive, in the wisdom of nature, that arteries may be expanded on the coats of the stomach in such manner as to perform secretions, as that they should be united together by cellular membrane, in the form of glands for the same purpose.

Many persons are at present of opinion, that the gastric juice contains an acid. From the experiments of the Abbe Spallanzani, from those of his colleague Professor Scopoli, and indeed from those of many others, I think we have good reason to doubt of the presence of a sensible acid in the gastric juice.

Having obtained some pure gastric juice from my own stomach, I found it capable of coagulating milk very readily. This however I conceive as no proof of acidity being present, as I have coagulated it with a solution of fresh runnet, in which I could not detect the smallest particle of acid. Nothing can be more erroneous than the opinion which prevails among some persons, that acids alone have the power of curdling milk. The truth is the very reverse of this; for it is now known, that the heart, lungs, and even liver of a turkey have been discovered to possess this property*. It will surely not be said that they are likewise acid. Again, certain vegetables, as the *galium luteum*, or ladies bed-straw, *vaillantia cruciata* or cross wort, *rubia tinctorum* or madder, *carduus* or thistle, *cynara scolymus* or artichoke, as well as many others, have been observed to have this effect. Indeed

* Vid. Spallanzani, Nat. Hist. vol. I. p. 271.

living fish have been observed to have this property; and Jacquin tells us *, that even lime-water produces an imperfect coagulation of milk.

Another fact, of no trivial import in the decision of this question, is related by Mr. John Hunter. This gentleman tells us, that 'in the sink calf, near the full time, there is no acid found in the stomach; although the contents have the same coagulating powers with those of animals who have sucked. † Now, as this coagulating property is evidently communicated to the stomach by the gastric juice, and as an acid could not be detected in the stomachs of these young animals, although they possessed this coagulating property, I think it appears sufficiently clear, that an acid does not exist naturally in the gastric juice. This fact I consider as conclusive, at least in as far as it prevents our being deceived by acids which are evolved in digestion.

Haller likewise appears to have been fully persuaded, that pure gastric juice does not contain an acid, and has quoted the authority of at least a dozen persons to prove his assertion. ‡ To determine this point however more satisfactorily, as it is of such importance in the adoption of a theory of digestion, I made the following experiment.

Deeming it necessary to obtain the gastric juice perfectly free from any extraneous matter, and likewise from any acidity of a former digestion. I kept a cat fasting eight and forty hours, after which it was killed. I found no food in its stomach, and but a small quantity of gastric juice. This I submitted to the usual tests for detecting the presence of an acid, but could not discover any. Hence I have been induced to conclude, that the gastric juice does not contain a sensible acid, and that whenever an acid is present in that fluid, it must either be the effect of disease or proceed from the remains of some former food.

Barry says, 'that the humours which are contained in the stomachs of the most rapacious birds, fishes, and beasts of

* Vid Element. Chem. Treat. de lacte.

† Vid. observations on the animal œconomy. p. 163.

‡ Vid. Element. physiol. Tom. VI. p. 143.

prey, have never an acid, but a saline taste.'‡ And from the chemical analysis of the gastric juice of the crow, by Professor Scopoli, we find he could not detect the presence of an acid in that fluid, but discovered it to be composed of, first, pure water; secondly, a saponaceous and gelatinous animal substance; thirdly, sal-ammoniac: and fourthly, an earthly matter similar, he says, to that which exists in all animal fluids.

We come next to consider the solvent power of the gastric juice, which is the great basis on which the whole of the present favourite theory of digestion depends.

Spallanzani tells us, that this fluid in a dog, not only acts on bone, but even corroded the dense enamel of two dentes incisores taken from the upper jaw of a sheep.* And from experimenting on himself, he found he could digest not only 'muscular fibres and membrane, but tendon, cartilage, and even bone itself, when not too hard.' From the excellent inaugural thesis of Dr. Stevens, we likewise learn, that various animal and vegetable substances, were dissolved by the gastric juice of the human stomach when inclosed in spheres. In like manner bone, and even ivory spheres were dissolved, which he had introduced into the stomach of a dog.

Since the publications of these two ingenious gentlemen, and particularly since the paper of Mr. John Hunter has appeared, further investigation of the solvent power of the gastric juice seemed unnecessary. This gentleman discovered, that in those persons who had died of violent deaths, the stomach itself was corroded and dissolved at its great extremity, and accounts for the stomach not being acted upon during life, on the theory of a *living principle*. Several facts tend to shew, that there is something innate in living matter, which resists the action of the gastric fluid. It is well known, for instance, that worms exist in the human stomach unaffected by digestion while living, but are speedily acted upon by the gastric juice as soon as they are deprived of life. To account for this fact, we must consider the gastric juice as a mere chemical solvent,

‡ Treat. on Digestion. p. 22.

* Vid. Natural History, page 211.

then by reflecting that all chemical solvents act by attraction, we may say, that the action of the vessels in living matter, is too great to be overcome by the attractive force of the gastric juice, and consequently their combination cannot take place. Another fact which shews, that the solvent power of the gastric juice is decidedly inert, as it respects some kinds of living matter, is, that there are several species of serpents, and particularly the rattle snake, who receive their young, on the least alarm, down their throats into their stomachs, where they have been known to remain for three and four hours. Fifteen or twenty of these young rattle snakes, have been found in the stomach of an old one, and not in the least injured by the gastric juice. * ‘A polype,’ too ‘inserted into the stomach of another polype, continues to live as before.’†

Although convinced from these facts, of the inertness of the gastric juice on these animals, still I was not satisfied of its action on other living animals. I therefore determined on an experiment with the gastric juice of a dog. For this purpose I kept a dog fasting twenty-four hours, at the expiration of which time I obliged him to swallow a number of small sponges. As his stomach retained these very readily and without any apparent inconvenience, I suffered him to remain at rest for three hours, immediately after which he was killed. From these sponges I obtained about half an ounce of very pure gastric juice. In it I immersed a common earthworm. The animal writhed about, and shewed symptoms of great distress. I corked the phial, and being at some distance from home, walked with it in my pocket to my residence. At this time I examined it again; exactly half an hour had elapsed from the time of my placing it in the phial. The animal shewed no symptom of life, and on taking it out of the phial, I found on its body evident marks of violent inflammation. Suspecting this, however, to be the consequence of the heat of my pocket, which (as it was in the month of April) I believe to have been about 80 of Fahrenheit, I repeated the experiment. The animal after being immersed

* Professor Barton's Lectures on Natural History.

† Spallanzani's Natural History, Vol. I. p. 111.

in the gastric juice, at the temperature of 70 of Fahrenheit, for fifty minutes, shewed no signs of life ; but there was no inflammation evident, as in the former. Knowing, however, that these animals live in a temperature still lower than this, I determined again to repeat the experiment. To effect this I placed an earth-worm in the phial of gastric juice as before, and covered it over with the same sod and in the same spot from which the animal was taken. Fahrenheit's thermometer stood in the turf at 60. In an hour I examined the phial, took out the worm, and found it lively and not in the least injured ; but on increasing the temperature to 70, it died as the last. Hence we may infer, that this animal also resists the action of the gastric juice while living and at its own temperature. How far this will be found to be the case with other animals, I am at present unprepared to decide. I am aware that according to the experiments of Dr. Stephens, a living leech which some persons have supposed to be an animal destitute of pores and capable of sustaining a degree of heat, equal to the human temperature, is digested by the human stomach.* But that they are capable of sustaining this degree of heat, I am not satisfied ; at least, of this we may be certain, that the temperature, at which they usually live, is not by any means as high. I had rather presume, that as in my experiments, the animal in this instance, had died previous to its being acted on by the gastric juice. Plot does indeed tell us, that he has seen the eyes of a carp and the nose of a roach, which were taken out of a jack-fish, digested, while they were yet alive.† And Cornelius is likewise said to have found a snake half digested in a bird's stomach, while life in that animal was still perceptible.

In all experiments with a view to ascertain the action of the gastric juice on different kinds of living animals, to proceed fairly I think we should keep this fluid exactly at the natural temperature of the animal immersed in it, otherwise it may be destroyed, either by an excess, or too great abstraction of heat. The gastric juice of different animals too, requires as

* Vid. his Inaug. Disser. pub. at Edin. in 1777.

† Vid. Plot's Staffordshire. Chap. 7. Sect. §7.

different temperatures to promote their action. Thus we learn that this fluid in amphibious animals and in fishes, acts on aliment at the common temperature of the atmosphere; but in most animals of the class mammalia, a higher temperature is requisite. The gastric juice of different animals varies in another respect still more considerably. Thus, in some animals, we see that it appears destined to act on animal food only; in others, on vegetables alone; and in others again, on both.

The gastric juice of man acts on antimony and copper; the former I relate on the authority of Professor Chaptall *, the latter on that of Professor Barton. This last gentleman mentions, in his lectures, the case of a child, who was much indisposed and salivated, in consequence of swallowing a cent; the cent when voided, was examined and found to be sensibly diminished.

According to Jacquin, Spallanzani and others, the gastric juice of itself has little tendency to either fermentation or putrefaction; but when mixed with other substances, its effect is rather to retard those processes.

From the numberless small arteries of the stomach, we may presume the quantity of gastric juice secreted to be very great; but like other secretions it is increased in quantity by stimuli, and particularly by the stimulus of food.

3rd. OF THE MUCUS OF THE PRIMÆ VIÆ.

This mucus is found on the internal surface of the stomach and intestines, and covers it very completely. It has the appearance, consistence, and properties of mucus in other parts of the body. It is secreted from small glands, which are situated under the villous coat of the primæ viæ. The quantity secreted appears to be very great.

Its use seems to be that of lubricating the surface of the primæ viæ, in order to facilitate the passage of their contents. It must likewise defend the internal surface of the stomach and intestines, from the action of the gastric juice, and from the acridude of the bile when regurgitated.

* Vid. Elemen. Chem. Vol. II. p. 260.

To ascertain whether this mucus has any effect, either in promoting or protracting the process of fermentation out of the body, I placed equal portions of leavened bread in two flasks, each containing eight ounces of water. Reserving one of these as a criterion, I added to the other about four drachms of this mucus, which I had procured from the stomach of a subject, who had died of a fall. Both flasks were placed in a sand bath, which was kept as nearly as possible at the temperature of the human body. In seven hours, the one containing mucus emitted air bubbles and smelled sour, whereas the one without mucus had no appearance of fermentation for several hours after.

4th. OF THE BILE.

The name of bile, has been uniformly given to a peculiar fluid, exclusively found in animal bodies, and which is secreted from a gland of a singular structure, called the liver. It is more or less of a yellowish-green colour, of a disagreeable bitter taste, of a thicker consistence and more plastic than saliva, of a singular aromatic smell when evaporated, which has been compared to that of musk, and which is by some thought agreeable.

Bile differs from all other secreted fluids in the body, in this, that it is not like them, separated from florid arterial blood, but is secreted from the dark coloured blood of the vena portarum, which is nothing more than a large vessel, made up by the concurrence of all the veins of the viscera of the abdomen, (except those of the liver) which empty their blood into it for the purpose of secretion. Now a plentiful supply of blood to a gland, being essential to the secretion of a fluid, and this blood being conveyed to the liver by the vena portarum, it has, and I think with propriety, been said, to perform the office of an artery. It ramifies every where throughout the substance of the liver, and terminates in two very different kinds of vessels. The one returns the blood, which is no longer fit for secretion, again into the general circulation ;

the others are the secretory vessels. In the former case, the extremities of the *vena portarum* inosculate with those of the hepatic veins, and thus the blood is returned to the inferior or ascending cava. In the latter, the secreting vessels soon terminate in the *pori biliarii*, by the union of which, in their passage out of the liver, trunks of a larger size are gradually formed, till at length they terminate in one of considerable magnitude, known by the name of the hepatic duct. This duct in its turn, soon terminates in another, which has gotten the name of the *ductus communis choledochus*, in consequence of its being the common duct of the liver and gall-bladder, through which bile is continually distilling into the duodenum, in which intestine this duct ends. Just however where the hepatic duct ends, and where the *ductus communis choledochus* begins, another duct arises, which extends to the gall-bladder, from whence it has received the name of *ductus cysticus*.

The secretion of bile, is a subject as yet involved in considerable obscurity. While some physiologists have considered it as the effect of a peculiar secretion in the liver, others of equal reputation, have asserted, that it is found formally in the blood. The correctness of these assertions are only to be determined by experiment, as therefore I cannot do this at present, I shall not venture a conjecture on the subject.

The bile being once secreted, it is received by the small branches of the hepatic duct called *pori biliarii*, from whence it passes into larger branches, till at length it gradually arrives at the great trunk of the hepatic duct; from thence it passes through the *ductus communis choledochus*, and is finally discharged into the duodenum. This is the most common course of the bile, but if from any spasmodic affection or morbid distention of the duodenum, or from any obstruction in the *ductus communis choledochus* its passage should be prevented, a retrograde motion of this fluid is the consequence. In this case it regurgitates through the cystic duct and finds its way into the gall-bladder, which is a very convenient receptacle, destined to prevent the hepatic duct from being surcharged with bile, in cases either of obstruction or of too great secretion.

Cystic bile differs, it appears, from hepatic, in this, that its consistence is more grumous, it is of a darker hue, and has a more pungent bitter taste. They do not differ in their constituent parts, but only in the proportion of those parts. In fact I do believe, that the only material difference which exists between them, depends on the more aqueous part of the cystic bile being absorbed, from its confinement and stagnation in the gall-bladder. Two causes concur to promote the discharge of cystic bile. The one is, the pressure which the gall bladder receives from the neighbouring parts, and particularly from the stomach when distended with food; the other, that either from the acrimony which bile acquires by stagnation, or from the mere stimulus of distention, a contraction of the muscular coat of the gall-bladder will be induced, and its contents will thus be readily propelled into the duodenum.

From the experiments of the most accurate chemists, the constituent parts of the bile appear to be, 1. A coagulable lymph: 2. A resinous matter: 3. Animal gluten: 4. Soda, and 5. A colouring matter, which is believed to be iron. Neither of these component parts of the bile is present in a perfectly free and disengaged state; they are all so combined as form an apparently homogeneous fluid. Bile, without the assistance of an intermedium, is not miscible, as far as I know, with any volatile or fixed oil, with animal fat, nor yet with butter or any other oleaginous substance. Neither does it appear to render these substances miscible with water, although perfectly so itself, I have seen some of the bile of an ox, which had been mixed in a phial with an equal quantity of olive oil, and which after having been kept for more than twelve months and frequently agitated, had not the least disposition to unite with it. The idea of its rendering oils miscible with water, appears to have arisen from its being long since used in the cleaning of stuffs, and hence it has been supposed to act chemically and in the same manner in which soap does. It does I believe act chemically, but still not in exactly the same manner in which soap does. Soap is said to have an attraction for both oil and water, and thus renders them

miscible ; whereas, bile having a greater attraction for the stuff to be cleaned, than oil has, only tends to displace it, and it is in this way that I believe it always acts, when used to remove spots of oil, or other greasy matter, from substances to which they are adherent.

The use of the bile in the animal economy is evidently material, and I may add, it is singularly important in the process of digestion. I do not however believe, that it has any effect in this process while going on in the stomach, as its presence in this organ is the consequence of regurgitation, and is without doubt morbid. This I infer, first, from the sickness of stomach, vomiting, vertigo and other symptoms of great morbid action, which attend its presence in that organ. And secondly, from these symptoms being speedily relieved, by such medicines as effect its discharge.

Doctor Monro caught having several frogs, at different times killed three of them, and as speedily as possible emptied the contents of their gall-bladders into the stomach of another. The consequence of this was, that the animal died shortly after.* I do not attempt to prove any thing more from this, than that there is somewhat deleterious in the bile of these animals, particularly as we are aware that the bile of other animals have been taken not only with impunity, but even with advantage. I have poured the recent cystic bile of a cat down the throat of a puppy, without his suffering the least inconvenience from it ; and I have seen a black servant whose taste had become so vitiated, after having accustomed himself to take the bile of an ox frequently as a stomachic, that he became fond of it, and so far from its proving injurious to him, he always thought himself much better after using it.

Bile neutralizes both the vegetable and mineral acids and is itself decomposed by them. In the duodenum, by mixing with the chymous mass discharged from the stomach, it first begins to separate the chyle from it, and being itself decomposed, its more aqueous part unites with the chyle, while its resinous adheres to the fæces giving them their natural yellow colour ; thus the former is prepared to undergo the

* Dr. Wistar's Lectures.

round of the circulation, the latter to be discharged from the body.

‘A further use of the bile,’ says an admired author, ‘is, to evolve and exterminate from the alimentary canal, the fixed air, which had been hitherto confined among the chymous mass.’* Again it prevents the too great accumulation of mucus in the intestines, and by its stimulus increases their peristaltic motion. Hence it is that biliary obstructions are generally accompanied with costiveness, and sometimes with a discharge of mucus.

The property of being both powerfully antiseptic and antizeumic, is likewise said to be possessed in an especial manner by the bile. MacLurg tells us, that ‘this fluid after having remained to his knowledge, for three days in a dead body, and although when the gall-bladder was taken out, there was a very offensive smell in all the abdominal viscera; yet this fluid, being poured into a phial, and closely stopped, acquired a sweet smell, which continued some days before the putrid fœtor began.’† This property of bile has been supposed to be intimately connected with its bitterness. Knowing however, that bile is secreted from the dark coloured blood of the vena portarum, (which is entirely free from any changes affected by air, through the medium of the lungs, and in fact, possessing all the characters of common venous blood) does it not appear probable, that its antiseptic quality depends on its attraction for, and consequent absorption of, oxygen. The opinion that the blood, by its circulation through the abdominal viscera, receives a putrescent tendency, appears to be erroneous, as it is deprived of its oxygen and consequently becomes antiputrescent; for bodies can only become putrid by the absorption of this gas.

The difference which exists between the blood of a fœtus, which is appropriated to the secretion of bile, and that of an adult, appears to be worthy of some attention. The difference is briefly this, the blood, from which the bile of a fœtus, is secreted, partakes more of the quality of arterial blood than that of an adult; this likewise makes a proportionate variation in the properties of the bile and consequently that of a fœtus

* Blumenbach.

† Vid. Treat. on the Human Bile. p. 76.

is of a more putrescent nature, or in other words, has less tendency to resist putrefaction.

The quantity of this fluid secreted seems evidently to be great, particularly when we consider the vast apparatus of its secretory organ, and the quantity of blood conveyed to it for this purpose. Indeed Dr. Monro tells us, that four ounces of cystic bile have flowed through an ulcer of the side daily. *

5th. OF THE PANCREATIC JUICE.

The juice denominated pancreatic, is a limpid fluid, which bears a greater resemblance to saliva than to any other fluid in the human body. It is secreted from a long and flat gland of the conglomerate kind, which lies under the stomach, and between the liver and the spleen, and which is known to anatomists by the name of the pancreas. The situation of this gland in the abdomen is transverse, being in the duplicature of the posterior portion of the mesocolon. It is found not only in man, but in most other animals, in quadrupeds, in birds, and in many fishes.

The external appearance of the pancreas, is that of one uniform substance, with its surface somewhat uneven from a considerable number of small convexities, and resembling very much in its structure the salivary glands. In the centre of the breadth of this gland, we find its great duct running in a longitudinal direction, and into which several smaller ones empty themselves on each side, like so many minute branches inosculating with one parent stalk. It has very properly gotten the name of the pancreatic duct, and opens generally in common with the ductus communis choledochus into the duodenum, for the purpose of emulging its contents. This however is not always found to be the case, as it sometimes opens by a separate duct into that intestine.

An exact analysis of the pancreatic juice, as far as I know, has never yet been made; but, like most of the fluids of the human body it is found to contain common salt and sal-ammoniac. The difficulty of procuring a sufficient quantity of this fluid, I believe is the reason, why its analysis has not hitherto been

* Vid. System of Anatomy. Vol. II. p. 389.

much attended to. We are directed to obtain it by inserting a small tube, to which a phial is appended, into the pancreatic duct of a living animal ; but this cannot be so readily accomplished, as we may at first sight imagine.

The use of this fluid is not perfectly understood. It is generally believed to have the effect of diluting the chymous mass, after its having passed into the duodenum from the stomach, and to assimilate it to animal nature. Likewise to dilute and attenuate the bile, which is sometimes too viscid and acrid.

From analogy the quantity of this fluid secreted appears to be very great ; as the pancreas is no less than three times as large as the salivary glands, and has every circumstance as favourable for the secretion of its fluid. Like other glands in the body, its secretion is increased by stimulants, which no doubt makes a very considerable variation in the quantity secreted at different times. The pressure which this gland receives from the stomach when distended with food ; the irritation of the chyme in its passage into the duodenum ; and even that of the bile itself, tends to promote the discharge of its juice. Like the bile, Haller says, * it is capable of being regurgitated into the stomach.

SECTION III.

HOW FAR SOLUTION IS ADMISSIBLE IN DIGESTION.

To assert that solution does not take place in digestion, would be to deny every thing like testimony in medicine. My attempt will only be to shew, why it is not probable that it can be the sole efficient cause of that process, and how it should be considered as tending to promote it.

By solution we can comprehend nothing more, than so minute a division of the particles of any matter, as to render that matter capable of being dissolved in a fluid ; and this too, without effecting any change of its component parts ; for no possible division be it ever so minute can have this effect.

* *Element. Physiol.* Tom. VI. page 309.

Thus, the component parts of water are the same, whether it be in the state of ice or of vapour. If this be a correct definition of what we understand by the term solution, a solvent in the stomach can have no other effect on our food, than merely that of separating it into very minute parts or particles; but this is very far from being all which takes place in digestion. Such a change must be effected on our aliment, for the due nourishment and support of our systems, as to convert it into that mild and bland fluid which we denominate chyle. This fluid differs in both its appearance and properties from the matters taken into the stomach, from which it was prepared, and consequently cannot be the effect of mere solution, by which operation matter may be divided, but I presume can never be changed in its component parts. Again, chemistry has not yet taught us that any difference exists between the chyle of carnivorous and that of herbivorous animals, and from the most direct experiment we learn, that two animals of the same species being fed, the one on the matter of muscular fibre, and the other on farinaceous matter, both afforded chyle in no respect different from each other*. Whereas, could chyle be produced by mere solution, it should surely differ in its properties, in proportion to the variety of matter from which it is prepared.

From these facts it appears, that the aid of some other operation is requisite to explain the formation of chyle. Perhaps several may be found necessary. In addition to solution, I do believe, that fermentation has likewise a very considerable effect. By it we know, bodies not only become decomposed and reduced to their elementary principles; but, by a recombination of those principles another substance is formed, differing materially from that from which it was obtained. It is by a similar decomposition and recombination of the elementary principles of food, together with its subsequent mixture with certain fluids of the *primæ viæ*, that I believe it becomes so far animalized and changed in its properties as to form chyle. That by the combination of bodies another is formed, differing in its properties from either of those of which it is composed, is too true to be denied. Thus, if we

* Fordyce on food, p. 143.

combined with a proper proportion of hydrogen and carbon, a certain proportion of oxygen, we obtain sugar, a substance differing very essentially from either of the other three. The effect of solution not being that of a change of the component parts of food, it is clear that its only operation in digestion must be, that of expediting fermentation. This opinion, if we judge from analogy, I presume will be found correct. In similar circumstances out of the body, the more minutely any matter is divided which is capable of fermentation, the more speedily will that matter go through its several stages. To return to our simile of sugar. If we dissolve this substance in water, its particles may be so minutely divided as not to be perceptible in that fluid, yet by evaporation the same sugar may be obtained, not at all changed in its properties. But if we suffer sugar to ferment, the result will be very different. It will be resolved into its elementary principles, carbonic acid will be disengaged, and we will likewise obtain alcohol. Here then is a decomposition and recombination of its elementary principles. Sugar being composed of oxygen, hydrogen, and carbon, and these elements being separated by fermentation, are re-united to form these two substances, to wit, the oxygen unites with a part of the carbon and is disengaged in the form of carbonic acid, while the remainder of the carbon is dissolved by the hydrogen and forms the alcohol. Thus we see the material difference in the effects of solution and fermentation.

SECTION IV.

WHY IT IS PRESUMABLE THAT FERMENTATION TAKES PLACE IN DIGESTION.

We have already considered digestion *a priori*; that is, we have investigated the causes which influence digestion. We have taken notice of the several properties of the matters which have the most material effect in this process. And we have likewise seen, that all the circumstances essential to fermentation, are possessed in an especial manner by food in the stomach. We have seen, for instance, that it is plentifully supplied with moisture, not only from our drinks, but

even from our saliva and the fluids of the stomach itself; that it receives a sufficient quantity of air from our saliva, by which fluid it is enveloped and continually conveyed into the stomach; and lastly, that its situation is admirably adapted to be supplied with the necessary quantity of heat; on all of which circumstances fermentation in a particular manner depends. We have also shewn, that saliva and the mucus of the *primæ viæ*, have a considerable tendency to promote this process.

Having proceeded thus far on our subject, it next becomes necessary, that we consider digestion *a posteriori*; or, in other words, that we attend to the effects produced by the digestion of food in the stomach. But in the first place, we shall say a few words on fermentation.

Fermentation is that great agent in nature, by which bodies are rendered totally different in their chemical properties, and which, from the variety of its products, has been long since divided into three several stages; to wit, the vinous, the acetous, and the putrefactive. From the first of these processes we obtain, *alcohol*; from the second *vinegar*; and from the third, *ammoniac*; by which means we are able always to ascertain, the nature of whatever fermentation has taken place.

It will be recollected, that these several stages of fermentation are capable of taking place, entirely independent of each other. Whenever the saccharine principle of any matter predominates, the vinous fermentation will take place; when the mucilaginous is most abundant, it will undergo the acetous; and when a greater proportion of gluten is present, it will run immediately into the putrefactive stage of fermentation. From this it appears, that on the several proportions of saccharine matter, of mucilage and of gluten, which any substance contains, depends the priority of the fermentation which will commence. Thus it is, that the vinous fermentation is capable of preceding the acetous, and *vice versa*. But they do not necessarily follow each other. Hence it is, that old and generous wines, in which the mucilaginous principle had been destroyed, are no longer capable of becoming acid, without the addition of a certain proportion of gummy mat-

ter*. Neither does milk afford a vinous spirit by its own spontaneous change, as in this case it loses its saccharine principle.

Different gases are disengaged, during the progress of these several stages of fermentation. The nature of these depend on the matter fermented. Thus, in the vinous and acetous stages of fermentation, carbonic acid is disengaged; while in the putrefactive, azote, carbonated hydrogen gas, sulphurated hydrogen gas, and phosphorated hydrogen gas, are all occasionally evolved.

We will now consider how far these gases, as well as the other products of fermentation, can be considered as being evolved in the *primæ viæ*.

In support of the first of these positions, we have that common fact, of our perceiving considerable eructations of air in affections of the stomach. Here it may be said, that the disengagement of air is the consequence of disease. To this I answer, that the eructation I consider as such, but not the formation of air. In affections of the stomach from gout, the quantity of air disengaged is sometimes very great, yet we cannot suppose that it is formed by this affection. I regret that I cannot from my own experiments, say any thing relative to the nature of the air, which is involved in the stomach during digestion. To place this matter, however, in as clear a light as I am able, I shall take the liberty of making a quotation from Plenck †. This gentleman informs us, that, ‘in a very healthy man, frozen to death by cold on a winter’s night, there was found a mixture of four kinds of air in the *primæ viæ*.

‘Fixed air was found in the greatest quantity in the stomach, and but little in the small intestines. Vital air was contained chiefly in the stomach, and small intestines, and,

‘Azote, and carbonated inflammable air, in the large intestines.’

To these I may add, that sulphurated hydrogen gas, and phosphorated hydrogen gas, have been proven to be disengaged *in crepitu*.

* Chaptal’s Chem. Vol. III, page 268.

† Treat. on the human fluids, page 141.

It appears from the works of Van Helmont, that he was the first person who suggested the idea of the presence of an acid in the stomach. His opinion has been assented to by Haller who relates, that the acetous fermentation is very prevalent in the stomach; that an acid is spontaneously evolved before putrefaction, and even sometimes resists that process. He indeed mentions an acid being detected in the stomachs of ruminating and of omnivorous animals, in those of birds and even of carnivorous animals. *

Most persons have witnessed the presence of an acid in their stomachs. But here an objection arises. It has been said, that whenever an acid is present in the stomach it is morbid, and indicates the diseased state of that organ. When accompanied with eructations, I believe this to be the case; as the quantity then appears to be preternatural, and is attended with an inverted peristaltic motion of the stomach, which is certainly the effect of morbid action. But we must not infer from hence, that the presence of an acid in the stomach is the effect of disease, as I hope to shew that it is detected in that organ, in the most sound and natural health.

This has been proven by Dr. Rush † many years ago. He has shewn us by several well directed experiments, that he always detected the presence of an acid in the contents of the stomach, when thrown up by an emetic, three hours after food was taken; but as it has been objected to his experiments that the acidity proceeded from the emetic tartar, which had been decomposed in the stomach, I shall endeavour to supersede this difficulty.

A gentleman in perfect health and capable of ruminating, dined on roasted beef, Irish potatoes and leavened bread. His drink was nothing but water. In four hours after, he brought up a portion of his dinner. It had acid taste, and turned a blue vegetable substance of a red colour.

The same gentleman having dined on boiled mutton, cabbage and leavened bread, and having drank water alone, as before, in four hours after, ruminated again. The portion of

* *Elemen. Physiol.* Tom. VI. p. 140. and 141.

† *Inaug. Dissert. de Coctio-Cib. in Vent.*

food brought up had an acid taste, and, as the last, turned a blue vegetable substance of a red colour.

These experiments were frequently repeated after having dined on different substances, and with uniform success. It was observed, that the acidity was not as perceptible to the taste in an hour or two after having dined, as it was in several hours after. These experiments may be readily repeated, as I do believe, that almost any person with a little trouble, may learn to ruminate.

Again, to determine whether an acid is evolved in the digestion of a cat, one was fed on boiled beef and Irish potatoes. In five hours after, it was strangled. Its abdomen was opened and its stomach taken out, having previously placed ligatures on its two orifices. The food had become soft and pulpy, and there was very little of any kind of fluid in the stomach. Some of this pulpy mass being placed in a glass vessel and mixed with a little water, shewed evident marks of acidity. It very speedily restored the yellow colour of paper stained with rhubarb, after its having been turned brown by an alkali.

I am not ignorant of the assertion of Dr. Fordyce, that in his experiments on dogs, cows, and sheep, he could not find the least trace of acidity in the duodenum; * neither could it have been expected otherwise, since, as has been already related, the bile has the effect of neutralizing acids, and of consequence as that fluid is almost always flowing into the duodenum, the quantity of acid must be unusually great to be detected in that intestine.

It is a prevalent opinion, that the acid which is present in the stomach is the phosporic, and consequently that it is not the effect of fermentation. But, as it is our duty not to admit either one position or another, unless it is supported by facts or experiments, I shall relate such of these as have induced me to presume, that it is not the phosphoric acid which is usually found in the stomach. And,

1st. The acid found in the stomach, does not precipitate sugar of lead from its solution in water.

* Vid. Treat. on Food. p. 150 and 151.

2nd. 'Being saturated with kali, that is, what was formerly called fixed vegetable alkali, it produces kali acetatum, formerly called regenerated tartar, or sal-diureticum.'*

Desirous of knowing whether an acid is evolved in the digestion of animal substances, as well as in the former experiments, the gentleman, capable of ruminating, dined on roasted veal alone and drank water as usual. In four hours after, a portion of the contents of his stomach was brought up. It turned a blue vegetable substance of a red colour, and had an acid smell and taste. I confess my being at a loss in this case to determine, whether the acid was the effect of the digestion of the veal, or whether it proceeded from the remains of some former food. It was my intention to have ascertained this point, by repeating the experiment on the same person, after his having subsisted on animal food for eight or ten days, but, as I have not now as many days previous to the delivering in of my piece, I shall be obliged of consequence to decline the idea. The following experiment however, will at least adduce probability, in favour of the acid having been evolved by digestion.

Having placed two drachms of roasted veal in a glass vessel, and covered it completely with saliva, it spontaneously became acid, long previous to there being any signs of putrefaction taking place. This fact is corroborated by the experiments of many authors of reputation. Haller takes notice of it.† Dr. Rush found that beef acquired an acid taste and smell, when exposed for two days to the heat of summer;‡ and Maclurg relates, that a mixture of mutton and water, passes through the acetous stage of fermentation before it putrefies.§

All animal matters when mixed with fermentable vegetable substances, have a tendency to promote fermentation, as appears from a number of experiments instituted by Pringle, and who likewise adds, that 'after such mixtures become sour they never return to a putrid state, but, on the contrary,

* Fordyce on Food. p. 148.

† Vid. Element. Physiol. Tom. VI. p. 316.

‡ Vid. Inaug. Dis. p. 21.

§ Treat. on the bile. p. 75.

grow more and more acid *. And yet, so far from our finding this ferment to be injurious to digestion, on the contrary, animal food seems to be the best adapted for the aliment of dyspeptic patients.

The publication of Dr. Wilson's ingenious inaugural dissertation on digestion, has induced many persons to suppose, that every idea of fermentation taking place in digestion is unfounded. The Doctor submitted to distillation, the contents of his stomach, brought up by an emetic four hours after food was taken, with a view of ascertaining, whether he could detect the presence of spirit of wine. This he could not, and from hence concludes, that fermentation does not take place in digestion. It will be seen that I have repeated the Doctor's experiment, and I am happy to add, with the same success; but I shall not draw the same conclusions.

Being in perfect health, I dined on corned beef, potatoes and leavened bread. My drink was water alone. In four hours after, I took twenty grains of ipecacuanha and brought up the contents of my stomach. It shewed evident marks of acidity. On submitting it to distillation, a transparent and limpid fluid came over into the receiver, which had a taste somewhat sweet, and an agreeable flavour; it had not the least appearance of spirit of wine, neither could I detect in it the presence of an acid. On examining however the *residuum*, which had not been evaporated to dryness, I was not a little surprised to find it still acid.

A dog was kept fasting for twenty-four hours. He was then fed for two days successively on animal food. Four hours and a half after taking his last meal, he was killed. The food in his stomach shewed evident marks of acidity. On submitting it to distillation, I obtained, as in the last experiment, a transparent and limpid fluid, somewhat sweet, with an agreeable flavor and in no respect different from that which I had obtained from the contents of my own stomach

From the above experiments we learn, first, that an acid was produced in digestion as in the former experiments, and that it was even found in the *residuum* after distillation; and

* Diseases of the Army, Appendix, paper 4, Eper. 28.

secondly, that spirit of wine could not be obtained from the food by distillation; but by no means that fermentation did not take place. I do not suppose that the vinous fermentation in a healthy stomach, is ever so complete that spirit of wine is formed, this would be equally as incorrect as to say, that putrefaction takes place in that organ. Neither can we suppose, that spirit of wine can ever be obtained from any matter, in which the acetous stage of fermentation has been completed and is then present.

We have already seen that the acetous fermentation is capable of preceding the vinous; it is probable this may be the case in digestion; nor would it be by any means singular. In the preparation of *koumiss* from the milk of mares, a drink much in use among the Tartars, the acetous fermentation always precedes the vinous. But admitting that the vinous fermentation does have the priority, every circumstance, to which our food is exposed in the stomach, must tend to hurry it on so speedily to the acetous stage, as to prevent the former from being at all perceptible. The heat to which it is exposed in the stomach, is greater than that which is requisite for the vinous fermentation; and this excess of heat favours the acetous stage.

When speaking of the acid formed during digestion in the stomach, the word *evolve* has been used by many persons; whenever I have followed them in this respect, it will be recollected that I have always meant, that it was evolved by fermentation. I do not suppose, that the acid in the stomach is simply disengaged from our food, as it is from the common *sumach* when mixed with water, or any other substance in which it is very abundant. The quantity of acid contained in the food, on which we have experimented, is not so great that this could have been the case; neither can it be obtained from it out of the body by any other means, than by the assistance of fermentation.

It now remains with the reader to determine, whether or not my position is correct, that this acid is the effect of fermentation.

Fermentation out of the body, differs from that which takes place in a living stomach, in this, that the former is

spontaneous ; whereas, the latter is induced by all the numerous circumstances in the stomach, which tend to promote that process, and of consequence must be more speedy.

Does not the fact of digestion being more speedy while we are at rest, than during exercise, favour the theory of fermentation ?

How shall we account for that warmth about the region of the stomach, so perceptible in some persons for several hours after death, unless we admit of fermentation ?

Whether we shall ever be so successful as to imitate nature in digestion, as in many of her other operations, I am unable to decide ; but, I flatter myself, time, that correct discernor of truth and error, will direct the attention of some more fortunate experimenter to this subject, and dissipate every doubt which may still involve it.

Having thus delivered my observations and experiments on digestion, in as concise a manner as my time would admit of, I shall now close this essay ; but to do this without an acknowledgment to the several medical professors of this university, would be a breach of that duty which my feelings claim.

To you, gentlemen, at least this small tribute of my esteem is due. Permit me then to present you with the sincere acknowledgments of a pupil, for the many opportunities of improvement which your lectures have afforded him. With the assurance of my wishes, that you may continue to enjoy all the pleasure of success in the science of medicine, I now bid you adieu.

EXPERIMENTS AND OBSERVATIONS

ON THE

ABSORPTION OF ACTIVE MEDICINES

INTO THE

CIRCULATION;

SUBMITTED,

AS AN INAUGURAL THESIS,

TO THE EXAMINATION OF THE

REVEREND JOHN EWING, S. T. P. PROVOST;

THE TRUSTEES AND MEDICAL FACULTY OF THE UNIVERSITY
OF PENNSYLVANIA,

FOR THE DEGREE OF DOCTOR OF MEDICINE;

ON THE EIGHTH DAY OF JUNE, A. D. ONE THOUSAND EIGHT
HUNDRED AND ONE.

BY BENJAMIN G. HODGE, OF THE WEST-INDIES,

HONORARY MEMBER OF THE PHILADELPHIA MEDICAL AND
CHEMICAL SOCIETIES.

Ore trahit quodcunque potest atque addit accervo.

Hor.

INTRODUCTION.

WHEN I first conceived the idea of writing on the absorption of medicines, I determined to take up the subject purely in an experimental point of view, without any reference or regard to the arguments that have been adduced, to prove either this or that thing. I observed the difficulty of drawing a conclusion upon many points involved in the subject, from such facts as are in our possession: so much are these facts opposed to one another, and so great the plausibility attached to each. This being the case, I was anxious to make some attempts of my own, to arrive at truth; and I was certain, it was only, (if at all) to be found, by making a direct and impartial appeal to the only true authority, to Nature herself. It soon appeared, however, that the subject I had chosen, was much more than I could possibly do justice to; was more copious than I at first imagined, and would lead to a much more numerous train of experiments, than the term allotted would permit me to make.

The difficulty, likewise, of procuring subjects for experiments, was a matter of no inconsiderable obstruction to our progress. The reader, therefore, will perceive, that we have advanced but a little way, and that too, in a field which has already been trod by others. The ground they have passed over, however, well deserves a more accurate examination. We can flatter ourselves only with having attempted this, by repeating a few of their experiments, and by shewing the proper degree of credit that ought, in our opinion, to be attached to others; and if, after all, the reader should be dissatisfied, let the trouble and inconvenience of experimenting, answer for our deficiencies. As it will be impossible, and even unne-

cessary, in the following dissertation, to notice every thing that has been said and done upon the subject, we shall relate only those particulars that appear to be the most important ; and in the course of such relation, we shall introduce the experiments we ourselves have made, together with such observations as may occur to us upon the subject.

The man, who would attempt to teach, or write, on subjects connected with physiology, without the aid of facts and experiments, had better relinquish the matter altogether. Here it is, by these guides, and not conjecture, that we must be conducted to truth. ‘ Of all the discoveries that have been ‘ made concerning the inward structure of the human body,’ says an elegant writer, ‘ never one was made by conjecture. Accurate observations of anatomists have brought to light innumerable artifices in the contrivance of this wonderful machine, which we cannot but admire, as excellently well adapted to their several purposes. But the most sagacious physiologist never dreamed of them till they were discovered. It is to these guides, therefore, that I mean to commit myself, in the following inquiry. But, alas ! what dependence can be placed upon the greater number of facts that are to be found in the records of medicine ? Who can draw the difficult line, that separates truth from error, in most of those particulars, that have been dignified with the name of facts ? We shall, perhaps, in the following dissertation, have occasion to observe, that the term has been much abused, and that it has but too often been employed to designate the dreams and errors of the imagination.

It would be unnecessary to offer any preliminary observations on the nature of the question, that is to be agitated. The importance of the subject, in a practical, and its beauty in a physiological point of view, will be too obvious to the physician and philosopher, to need the aid of eulogy to recommend it. Its utility will at once be made apparent, by referring the attention of the reader to the treatment of a common intermittent fever. In this disease, for example, if it could be correctly ascertained, whether the Peruvian bark exerts all its influence in the alimentary canal without going any farther ; or, whether it is absorbed into the circulation, and is there

enabled to produce in part its salutary effects, the practice, resulting from this knowledge, would be materially different. In the former case, the practitioner would throw in the medicine, only on the approach of a paroxysm; but, if the latter position were true, the patient would be obliged to take the medicine with no, or but little variation in quantity, during the whole of the intermission. A similar remark would apply to many other cases; and it is certainly owing much more to an ignorance in the *where*, than it is in the *how* medicines operate, that so much difference has prevailed concerning the time and mode of exhibiting them.

One thing I must request the reader to observe, which is, that the following experiments were not made, as is sometimes the case, to bend to the theory of the experimenter. On the contrary, when we entered upon the investigation of the subject, and had nearly advanced to the conclusion of it, we still believed in the absorption of active medicines: but, from the results of our experiments, and for other reasons which shall be given in the sequel, we have been necessitated to relinquish our former opinion. Whether the reasons assigned for so doing are sufficient, will be determined with more propriety by the reader.

We are conscious to what danger we expose ourselves, in attempting to oppose a doctrine that is become venerable by time, by talents, and by industry. But, from you, illustrious professors! (to whom I now address myself) from you we have little to apprehend; for, from a long acquaintance with you, both in a public and private way, we have much reason to believe, that prejudice forms no part of your characters; and that you are always willing to listen, with candour, and indulgence, to any attempt, the ultimate object of which is truth, that ‘jewel, which all good and wise men,’ together with yourselves, ‘are in pursuit of.’

EXPERIMENTS AND OBSERVATIONS

ON THE

ABSORPTION OF MEDICINES, &c.

THE notion, that medicines, in order to be effectual, must be absorbed into the circulation, in their active states, took its rise at an early period. It was the very soul of the humoral pathology. As this doctrine referred all the phenomena of disease, to particular dispositions of the fluids, so it was imagined, that medicines could do no essential service, unless they were applied to the seat of the disease. These theories have been, in a great measure, corrected; the former, by a more improved pathology; the latter, by a nicer attention to the effects of medicines on the body. In consequence of this accurate attention, physicians soon discovered the insufficiency of the above explanation; and found themselves obliged to seek for some mode of accounting for several particulars respecting the operation of medicines, more rational and satisfactory, than the one by absorption. That exquisite, though mysterious law of the animal economy, by which one organ is rendered sensibly alive to the feelings of another; by which impressions on one part, are immediately propagated to others, the most distant in the body, presented itself to their consideration; and this principle, under the various names of *vis medicatrix*, consent, sympathy, &c. has long been considered as a chief agent of the business in question.

But, although the influence of this sympathetic connection has been acknowledged, physicians have never departed entirely from the old doctrine. They have always continued to believe, that medicines, in general, are absorbed into the circulation; and that this, in many cases, is even necessary to their proper action. Thus Dr. Cullen, who was a great advocate for the notion of sympathy, nevertheless observes, ‘ In many cases of increased evacuations, it is indeed pretty evident, that the medicines, exciting the evacuations, are actually conveyed and applied to the secretories or excretories of the parts concerned:’ and, the absorption of mercury into the constitution is, with many, deemed absolutely necessary in the cure of lues venerea; not to mention many other instances.

This opinion, however, is at present totally denied by some physicians, who attribute the effects of all medicines to sympathy alone.

The supporters of this heresy, among whom I may include myself, declare, that no active medicine is ever taken up into the circulation. That opium, camphire, &c. cannot, nor have they ever been proven, to circulate, as camphire and opium, in the blood vessels of a living animal. They think it would be incompatible with the life of the animal, that such active substances should be absorbed into the circulation; since milk, and other bland fluids, are known, when injected into the vessels, to occasion immediate death. They argue further, that, as the powers of sympathy are acknowledged, it would be useless and unphilosophical to admit of more causes than are sufficient to explain the phenomena; and that it would, moreover, be contrary to the general simplicity of nature, who never employs two instruments to accomplish a single purpose. This new theory, therefore, resolves itself into the following form, and supposes, that every medicine, ‘ When received into the stomach, after the first impression on the very sensible coats of that organ, the nature of it is gradually changed by the solvent powers of the gastric juices; or, if incapable of being digested into a mild and nu-

trititious chyle, it is carried through the intestinal canal, and ejected as useless and noxious to the body.*

These objections are founded, not on hypothesis, but in matter of fact; and if the inutility of a thing, be a proof of its falsity, we should have little trouble in establishing our point by shewing from many circumstances, that absorption is not only insufficient, but that it is perfectly unnecessary, in explaining the action of medicines. The following facts would suffice for this purpose:

When the body is in a state of langour and debility, one glass of wine, will immediately restore to the muscles their accustomed tone; will revive the drooping spirits, and will add new force and vigour to the whole arterial system. A tremor of the hands in drunkards is often lessened, or removed, by a dram, or some strong wine; and that, in so short a time, as entirely to prelude the possibility of absorption. Some medicines, as camphire and opium, have been found in the stomach, without any diminution in quantity, long after they have produced their peculiar effects. ‘Frogs,’ says Girtanner, ‘which live a long time after the heart is cut out, and which are consequently deprived entirely of blood, are killed as quickly by the poison of the viper, as if their blood had not been let out.’ And the celebrated professor Whytt has ascertained, by experiment, that if the heart of a frog be taken out, and a solution of opium injected into the abdomen, the animal will be convulsed in a very short time. Dr. Barton, in his valuable lectures on the materia medica, says, he once stopped an hemorrhage in a distant part by the exhibition of only two grains of *sac-saturni*. Medicines not only can be shewn to produce their effects on distant parts of the body, by a mere impulse upon the part to which they may be applied, but there are many, whose operations are alone explicable upon this principle; many that can act in no other way; a striking instance of which is to be found in the cure of buboes, hernia humoralis, &c. by emetics. We might thus go on to multiply proofs of the same nature, and to shew, that the effects of every class of medicines, as sialogogues,

* Percival, Operation of Medicines.

diuretics, diaphoretics, &c. are all referable to a sympathy between the different parts of the system. But, perhaps, we have already insisted too much upon the establishment of a principle, which few people, in the present improved state of medicine, will be hardy enough to deny.

Some gentlemen, more partial to their own theory than to the facts of others, have endeavoured to raise objections to the experiments, concerning the injection of milk and other matters into the circulation, and to destroy the true deductions that ought to be formed from them. This they do, by observing 'that what passes along the lymphatics or lacteals, is carried into the thoracic duct, and there mixed with a large portion of chyle or lymph, by which its acrimony is sheathed and diluted, or its chemical properties changed, before it enters the blood*'. Such objections, however, do not, by any means, appear to be sufficiently founded. For, in the first place, simple dilution does not alter the nature or properties of any substance whatever. A given quantity of wine will produce similar effects, whether it be taken into the stomach in a diluted, or undiluted state. And, in the second place, as it is well known that the chyle mixes with, and becomes blood itself, very shortly after it is poured into the blood vessels, the milk, and other foreign matters united with it, would then be deserted, and would be left in that same undiluted state, in which they have been injected into the vessels, and in which they kill. As to the chemical change, which these foreign matters are supposed to undergo, in the lacteal and lymphatic vessels, that will be readily admitted. It is just one of the propositions we wish to maintain. We wish to establish, that all matters, capable of forming chyle, must be reduced to that state previous to their entrance into the circulation; and if they will only grant this, they shall have our free consent to call the change, a chemical one.

If therefore, any substance causes death, &c. when injected immediately into the blood vessels, it follows, of necessity, that that substance, provided it be in the same form, will always produce the same effect, let the mode of introducing

* Percival.

it into the circulation be ever so much varied. We will avail ourselves of this reasoning on a subsequent occasion, to extricate us from a difficulty. We do not mean, however, to insinuate from this, that some other substances may not be thrown into the blood-vessels, and may not exist there without much danger. This inference would be opposed by facts, and one of a very remarkable nature is related by the celebrated Fontana *; but such experiments only prove the possibility of the absorption of those substances, and with possibilities we have nothing to do.

The supposition, that active medicines are taken into the circulation, appears to owe much of the credit it possesses, to the very mechanical manner in which the lymphatics are supposed to absorb and transmit different articles into the blood.

Not being acquainted with any other modes of explaining the rise of fluids in tubes, but by capillary attraction, and the formation of a vacuum, philosophers availed themselves of these principles, to account for the same phenomenon in the absorbents. The explanation was a very natural one. Unfortunately, however, they forgot, in this, as in many other cases, that they were reasoning, not on dead, but living matter; and that analogies, drawn from the one, would not always be applicable to the other. Thus it is not only difficult to imagine how the soft, delicate, and yielding extremities of these vessels can form themselves into capillaries, &c. but, from many circumstances, it is rendered pretty clear, that they evince something like design in their operations. They shew a greater aptitude to receive some substances, than others, that are reduced to the same form. They absorb the nutritious, in preference to the other contents of the alimentary canal, which would not be the case, if they acted purely on those mechanical principles. Neither will these principles explain the manner in which they carry away the hardest parts of the body, such as bone, cartilage, muscle, &c. unless, indeed, we were to assume what is not proved, viz. that these parts undergo a solution previous to their absorption. But

* Fontana on Poisons.

the amazing power of the chylo-poietic viscera, among which the lymphatic system must be included, appears to be the most material objection to the absorption of medicines, in their active forms. From that almost nameless variety of substances, which the caprice or necessity of man has induced him to take as aliment, only one combination results, viz. chyle. The absorbents are capable of selecting, amidst so great a variety, such principles alone, as they can afterwards convert into chyle. The chyle of different species of animals, differs in no respects, although their aliment is, apparently, the most opposite in its nature. The absorbents appear, further, to be the most pre-eminent of the chyloform organs, for they not only produce chyle, from such matters as have suffered the action of the stomach, (for chyle is not formed in the stomach or intestines) but they possess the power of absorbing, and operating the same change upon milk and other nutritious articles, that may be injected into the different cavities of the body, as the rectum, the abdomen, &c. In these cases, the absorbents appear to have the whole business of digestion in their own hands. This chyloform power in the lymphatics, may be shewn, by an analogy derived from the animal and vegetable kingdoms. The whole tribe of vegetables are intirely destitute of a stomach. Some animals, of an inferior order, are likewise formed without this organ. Now, in both these living productions, there can be nothing to digest their food but the absorbents; and yet, the alimentary matters, they take in, are most certainly digested.

The only proofs, then, upon which the absorption of medicines rests, are the appearances of those substances, in the secretions, and in different parts of the body, after they have been taken into the stomach, &c. Facts of this nature are plentifully scattered over every work that relates to medicine. Indeed, I recollect but very few substances, not only in the materia medica, but even in Nature herself, that have not, at one time or other, been discovered to pass through the circulation. But it must not be supposed, that whenever an author relates facts of this kind, that he always does it upon the evidence of his own senses. As the absorption of medicines was universally believed, and looked upon as a medical axiom till

lately, physicians copied from one another with as much confidence, as if the circumstances they related were actually before their eyes. Thus, although few men in modern times, have had the happiness of witnessing, as Etmuller did, the presence of 'simple water wine, and wine with sugar, and emulsions,' in the identical unchanged state, in the urine, in which they were taken by the patient; yet, we find his facts frequently mentioned, and that too, by some of the most respectable modern physicians. As some of these facts, however, are handed down under the protection of names, that will always command our respect and veneration, we must now proceed to consider them.

There was no subject wherein a greater unanimity prevailed among physicians, than that iron and its various preparations, were absorbed into the circulation. The opinion was grounded upon many plausible circumstances. Iron, it was well known, existed as a component part of the blood, and was supposed, likewise, to be the cause of its red colour. It was, and, I believe, continues to be, a general observation among practitioners, that this colour is increased by the use of martial medicines. Mr. Menghini even found, by experiment, that the blood of persons, who take martial preparations, contains more iron than it does in an ordinary state; and Mr. Lorry observed, that the urine of a *sick* person, to whom he administered iron in a state of extreme division, was coloured with the nut-gall.* Many modern physicians have altered their creed upon this point, chiefly in consequence of an experiment made by the ingenious Dr. Wright, wherein this gentleman was unable to detect any iron in the thoracic duct of a dog, to whom he had given an ounce, and an half of sal martis.† Such contradictory experiments produced much doubt in my mind, especially as I was induced to believe, that, if any foreign matter could gain admittance into the blood, except in the form of chyle, it would certainly be iron. In order, therefore, to make up an opinion on the subject, I had recourse to the following experiments.

* Chaptal.

† Philosophical Trans. Abridged.

EXPERIMENT.

One drachm of green vitriol, finely powdered, was wrapt up in some meat and given to a dog. In about an hour and a half after this, he vomited a little; but whether he threw up the whole quantity, I cannot decide. I suspect he did not, since so considerable a time elapsed, before the vomiting commenced. However, as soon as I perceived this, I dissolved four drachms of the same medicine in a quantity of milk, and injected it into his rectum. In the course of half an hour afterwards, I had the misfortune of seeing this voided likewise. I then dissolved near an ounce more of the medicine, in some more milk, which I again injected into the rectum, taking, at the same time, effectual precautions of preventing a return of it. In an hour and an half after this, the dog was killed, and several drachms of chyle were obtained from the thoracic duct. To one portion of this was added some of the prussiate of potash; to another, the alcohol of galls. No change of colour, however, ensued in either case. But when I dissolved the sixth of a grain of the vitriol, in a portion of the same chyle, my tests discovered the presence of it immediately. Quantities of the serum and urine were tried in the same manner, but without discovering the presence of iron. Mr. Jacobs assisted at this experiment, who agreed to every thing I have related.

EXPERIMENT.

Having procured a dog, and kept him starving for several days, I poured down his throat two scruples of a solution of sal-martis; at the same time, half an ounce of the same medicine, dissolved in a sufficient quantity of milk, was injected into his rectum. In the space of fifteen minutes, the dog vomited a little, and was affected with a violent tenesmus, without being able, however, to discharge the contents of the rectum. In about an hour and a half the dog was killed, and the thoracic duct secured by a ligature. After the duct became distended, I punctured it, and collected a quantity of chyle sufficient for the purposes of experiment. This being

divided into separate portions, suffered no change of colour, upon the application of the different tests formerly mentioned. A white coagulum always took place in the chyle when tested by the alcohol of galls. After the experiment was finished, and we were about to leave the room, the propriety of examining the mesenteric glands, was suggested by an ingenious friend. For this purpose, I detached a number of these glands, which being cut through, in order to expose their internal surface, were immersed, in separate portions, in clean water. To these glands, thus circumstanced, were added sufficient quantities of the alcohol of galls, and the prussiate of pot-ash. Soon after, a black colour in the one, and a blue in the other, took place, evincing thereby, the presence of iron. At first, I supposed this circumstance might have been owing, either to the knife made use of in dividing the substance of the gland, or to some particles of green vitriol accidentally insinuating themselves into the water. That it did not depend, however, upon this latter cause, must appear evident from this, that the colours produced, were confined wholly to the substance and interior structure of the glands.

But to be thoroughly satisfied upon the subject, I repeated the experiment above half a dozen times, taking care to avoid the knife, and every circumstance that could influence, or could be adduced as an objection to the experiment. But the results were the same in every instance. Did I perform the experiment too soon for the iron, thus absorbed, to pass through the gland, and to arrive at the thoracic duct? Would it not have discovered itself in the duct, if sufficient time had been allowed? This looked extremely probable, and a wish to see how far it was true, brought about the following experiment.

EXPERIMENT.

Having kept a dog starving two days, I offered him half a drachm of green vitriol, dissolved in a portion of milk, a part of which only he took: half an ounce of the same medicine, dissolved in another portion of milk, was injected into his rectum. After remaining three hours and a half, he was

killed, and a quantity of chyle collected from the ductus thoracicus, which did not produce by the addition of the usual tests, any colour, that was in the least indicative of the presence of iron. Our next object, was to examine the mesenteric glands: and here the same phenomenon occurred, as in the last experiment, only the colours were more intense in the interior structure of that part of the mesentery which intervenes between the glands and intestines. The liquor of the thoracic duct exhibited a very different appearance in this, from what it did in the preceding experiments; instead of a white milky colour, it resembled serum that is slightly tinged with the red part of the blood. To ascertain whether this iron in the glands was really owing to absorption, or whether it was the mere effect of transudation taking place after death, I had recourse to another experiment.

EXPERIMENT.

Half an ounce of green vitriol was injected into the rectum of a dog. Two hours afterwards he was killed, and immediately, before transudation could take place, several of the mesenteric glands were taken out. Separate portions of these were tried by the different tests, while another portion was put into simple water to serve as a comparison. A black colour was produced in that gland to which the alcohol of galls was applied. It was not quite so evident as in the former experiments. No change of colour took place in that portion tested by the prussiate, except in a small part of the mesentery, which adhered to it. That portion of gland placed in water suffered no change. Some chyle was taken from the thoracic duct, but it was not found to contain any iron. I then wished to see if the black colour produced in this experiment, were the effect of the alcohol of galls, independent of any iron.

EXPERIMENT.

Some glands were taken from the mesentery of a dog, which had served for an experiment of a different kind, but which had taken no iron. These, however, suffered no

change of colour, by the application of the alcohol of galls to them.

With regard to the absorption of the iron, as far as the glands, it does not appear that any weight can be attached to the circumstance. For it must be recollected, that in every instance an immense derangement was brought on the intestines ; the colon and rectum always looked as if they were fast tending to gangrene, and in some cases they were actually in that state, being as black as my hat.

These experiments, then, not only prove the absence of iron in the chyle, but likewise, that of the acid with which it was combined ; for, if the sulphuric acid had been present, it would have formed a blue colour with the prussiated alkali. But it may be objected, that they are inconclusive, inasmuch as they ' only evince, that iron did not subsist in the chyle as a vitriol, qualified to strike a black colour with galls,' &c. This objection may be an ingenious one, but it certainly is not founded in a knowledge of chemistry. For, contrary to the assertion of Dr. Percival, if iron had existed there, either as iron, or as an oxyde, or in any other form, it would certainly have been discovered by the above means.

But how are we to reconcile the experiments of Menghini and Lorry, formerly mentioned, and likewise, the more intense colour of the blood, observable in patients under the use of iron ? In answer to this, I must take leave to observe, that these facts, even when admitted in the fullest extent, will not, by any means, establish that opinion, which, on superficial examination, they are so eminently calculated to do. The subject respecting the existence of iron in the blood, is well known to be involved in much obscurity. How its presence there, is to be explained ; what purpose it answers ; how far it is subject to variation, in point of quantity ; and, if subject to variation, with what particular states of the system, an increase or diminution, in its quantity, may be connected ? are queries, which cannot be answered in our present imperfect knowledge of the business ; and until they are, these experiments will be of little value. It is not our business to account for the presence of iron in the blood, but it is most probable, that it is a compound substance, formed by a peculiar process of organi-

zation, and is intirely independent of absorption. In this way, it appears to be formed in vegetation; for experiments shew, that it exists in vegetables, that have fed entirely on air or distilled water†. It is pretty evident, however, that this iron may exist in very different quantities at different times. Indeed, why cannot this take place with the iron, as it does with regard to the other component parts of the blood? The relative proportions of these are well known to be constantly changing. The serum is at one time abundant; at another, it exists in small quantity. The red globules are now more, now less; and the same thing is true of the coagulating lymph. Iron may, therefore, be found in the blood in larger proportions at particular times, and that too, when the patient is under the use of iron: yet it does not follow, that this increase in its quantity is necessarily owing to the absorption of the medicine. The use of martial medicines may only be the indirect cause, by producing that state of the system on which an increase of iron in the blood depends. As to the colour of the blood becoming redder by the use of iron; I think this is more liable to objection than the question we have just been considering. If the colour of the blood really depended on iron, the circumstance, I acknowledge, would go a great length to prove the absorption of this metal. But sufficient evidence is still wanting to shew the truth of the position. The greatest physiologists, 'tis true, have agreed in attributing the redness of the blood to the iron it contains; 'but when we reflect how many various colours iron gives in its various states; when we reflect, that the unknown cause, which gives colour to the iron, may give colour to the blood; when we reflect, that of this crocus of iron, we can hardly procure one poor grain from four hundred grains of these red particles of the blood; we cannot but be conscious, that this peculiarity is not yet explained.* Besides these objections, it may be presumed, that the improvement, both of the appetite and of digestion, and a more vigorous circulation, all which result from the exhibition of iron, are circumstances as

* Chapthal, Abernethy.

† John Bell.

satisfactory in explaining the phenomenon, as any unknown cause that could be alledged.

It is unfortunate for medicine, that theory is so often mistaken for truth, instead of being considered as the mere creature of uncertainty.

Despairing of the possibility of ever introducing iron into the circulation, I determined to make the attempt with some other substances.

The power of stopping hemorrhages in distant parts of the body, and some other particulars, respecting the operation of astringents, have entitled this class of medicines, in the estimation of many, to the privilege of being absorbed into the circulation. To arrive at some degree of certainty on the subject, the following experiment was instituted.

EXPERIMENT.

Half a drachm of the powdered nut-gall, concealed in a quantity of meat, and three ounces and a half of a saturated aqueous solution of the same medicine, mixed with some milk, were taken by a dog, that had been starved for several days. It may be necessary to observe, that the dog did not vomit, nor did he appear to be affected in any respect whatever. About two hours afterwards he was killed, and a large portion of chyle collected from the thoracic duct. But this did not exhibit the least appearance of astringency, by adding to it a solution of the sulphate of iron. The chyle, in this instance, could have been formed only from the aliment which I had just given the dog, and with which I had combined the galls. Notwithstanding this, however, it did not contain a particle of the medicine.

Among the many substances, concerning the absorption of which, physicians have been generally agreed, lead must be mentioned. The insidious manner in which this metal undermines the constitution, favours much of absorption. The change of colour which it causes in the muscles of paralytic limbs, has likewise been supposed to depend upon the same principle. And professor Thunburg, who was salivated by

accidentally taking a large quantity of ceruse, mentions, that the lead was perceptible in his saliva *. It were to be wished that the professor had been a little more particular, and had ascertained the presence of the lead by some better criterion than that of taste. But although these circumstances are not at all conclusive, yet they were sufficient to create a strong suspicion in my mind that I should find them realized by experiment.

EXPERIMENT.

I kept a dog confined, and whenever I fed him, I mixed a portion (generally about fifteen grains) of *sac. saturni* with his food. The dog bore this regimen very well at first; but after some time he grew sick of the medicine, and commonly threw it up, so that I was obliged to desist. By this practice, I forced him to take, altogether, not less than sixty grains, exclusive of that which he vomited up. Half an ounce of the same medicine was immediately injected into his rectum, and proper precautions taken to prevent its expulsion. After two hours had elapsed, he was killed, and a quantity of the liquor of the thoracic duct collected, which did not shew the presence of lead, when tested by the phosphoric acid, and the sulphurated hydrogen gas. Neither did the serum of the blood evince the existence of lead in it by the same tests. Here lead did not exist in the chyle or blood, either as sugar of lead, or as an oxyde, for if it had been present in those states, it would have been rendered sensible. The glands of the mesentery did not contain any of the medicine.

My next object was, to ascertain whether mercury could be found to enter the circulation, since it is upon this subject particularly, that the doctrine of absorption has rested its greatest support.

The authorities in favor of the absorption of this metal, are numerous and nearly as respectable. Our credulity, indeed, is often startled, especially when we are informed, that upon opening a vein some drachms of it have flowed out

* Voyage to the Cape of Good Hope.

with the blood *, &c. But, after all, it must not be denied, that cases of a similar nature have been handed down to us by men of great eminence in the science. Boerhaave, Haller, Mead, Brodbelt, and a long catalogue of others, have *seen* globules of mercury in the bones. Added to this, is a case, related by Dr. Cooper, of a woman who was salivated, producing the same affection in her sucking child. This last fact, however, deserves very little consideration; for, if mercury was used externally in any form, that circumstance alone would be sufficient to explain the mystery. But in opposition to the above facts, we have the experiments of Dr. Slare, which were made on the saliva of a lady in a state of salivation without discovering the smallest quantity of mercury †. Dr. Saunders, ‘by various and accurate tests, could not discover, in the secretions, any mercury in persons under a salivation, either from the internal or external use of it ‡.’ Some experiments of a similar kind, which I have made, were marked with similar results; but as they may answer some useful purposes, and may probably tend to guard others from a deception which I was near falling into myself, I shall relate them.

EXPERIMENT.

Several ounces of saliva were procured from a patient in a high state of salivation. In this were immersed several pieces of gold, silver and copper, which were allowed to remain two days. At the end of that time, no amalgam was formed on them. Supposing that if there were any mercury, it would more readily unite with other metals, when it was in a state of vapour.

* Mead’s Works.

† History of the Royal Society.

‡ Saunders on the Liver.

EXPERIMENT.

I took the same saliva, and after suspending a bit of gold just above the surface of it, it was subjected to heat and entirely evaporated; but no amalgam appeared on the gold. I was apprehensive that these experiments were made on too small a scale. To obviate this objection:

EXPERIMENT.

A quart of the same saliva, being slowly evaporated to a thick consistence, produced no change of colour in a bit of gold that was immersed in it a whole night.

EXPERIMENT.

By the favour of.....I obtained several ounces of blood from a patient, in a high state of salivation. After it had separated into serum and crassamentum, I placed in the bottom of the bowl containing it, a piece of the purest gold, and allowed it to remain there a whole night. But no amalgam was formed on it.

EXPERIMENT.

A part of this blood, being placed in an oil flask, with pieces of gold and copper suspended on the surface of it, was exposed to heat. The gold did not suffer any change, but in a short time, the copper became as white as silver, and some persons, very capable of judging, declared it to be the effect of amalgamation. As the gold, however, was not affected, some doubts still remained, which I thought, could only be cleared up by a comparative experiment.

EXPERIMENT.

I, therefore, obtained some blood from a person, who was not under the use of mercury, and by treating it as in the last experiment, the same appearance precisely took place. The copper was whitened. Had not faith in the experiments of others, and some degree of scepticism, induced me to make

this trial upon the blood of a healthy person, I should have been most miserably deceived.

The deduction, then, that must be formed from these and other experiments, is, that mercury does not exist in the circulation or secretions in a pure metallic state. But they go no further; for, if mercury had been present, either as an oxyde, or in combination with an acid, it is evident, it would not have been detected by the means made use of; and proper experiments, with a view to find it in those states, have not, as far as I know, been made.

But do not the depositions of fluid mercury (supposing for a moment that such was ever the case) in the bones, &c. shew that it must have existed in that form in the circulation? I answer, no. For, that it does not, is evinced by what has been said; and that it cannot is proven by a fact, related by Saunders, who injected two drachms of mercury into a vein; and thereby killed the dog, that was the subject of the experiment.* If such depositions can be accounted for at all, it must be by supposing, either that the mercury is reduced from its combinations in passing through the extremities of the arteries, or, that this takes place after these combinations have been deposited. The only thing that remained, was to ascertain, whether mercury existed in the states just mentioned; for which purpose the following experiment was made.

EXPERIMENT.

I procured a glass tube, hermetically sealed at one end and open at the other. Into this was introduced several drachms of the dried blood and saliva of the same patient. A bit of gold was placed over a small hole, left in the open end of the tube, to prevent any mercury from passing off undetected. The tube was then exposed to a red heat, and after remaining there a sufficient time, we could not discover that any mercury had either passed off in vapour or was present in the tube.

The contents of the tube exhibited much the appearance of globules of mercury. This appearance occurred likewise in a former experiment on the blood, and, in both instances.

* Saunders on the Liver.

was near leading me into an error; but when attentively examined, they were found to be globules of air instead of mercury. So great was the resemblance, that these globules were, at first sight considered by many, even the professor of chemistry himself as mercury. Will not this serve to explain those appearances, which, on dissection, have been attributed to mercury? 'Heaven knows how seldom things are what they seem!' one thing I am certain of, that if any person, prepossessed with the belief that mercury is absorbed, had seen these experiments, he would, without doubt, have pronounced them to be globules of mercury. I shall conclude this subject with a quotation from Mr. Hunter. 'It may be supposed unnecessary to mention, in the present state of our knowledge,' says this great man, 'that mercury never gets into the bones in the form of a metal, although this has been asserted by men of eminence and authority in the profession; and even the dissections of dead bodies have been brought in proof of it; but my experience in anatomy has convinced me that such appearances never occur.' *

Another circumstance, that has been urged by the supporters of absorption, is, that animals, which are at one time innocent and wholesome, will become poisonous, in consequence of eating certain poisonous matters. The fact, I believe, is strictly true. We ourselves have witnessed it in the fish called the sprat, in the West-Indies, and in confirmation of the same thing, may be found, in the memoirs of the London Medical Society, an excellent paper on the subject, by Dr. Thomas. This author, in speaking of the poisonous quality of the sprat, and some other fish in the West-Indies, observes, 'that it arises, however, from their food, is strongly corroborated by several circumstances; but what that food is, remains yet to be discovered. It is a well known fact, that the land-crab, (*cancer-terrestris*) when taken near manchineel trees, is found, particularly in dry seasons, at one time safe, at another, poisonous, from feeding on the bark or leaves of that tree in lieu of other nourishment.' Pheasants are said to acquire a poisonous quality, by eating, from neces-

* Hunter on the Venereal.

sity, the fruit of a certain plant. A fact, of the same kind, is related in Foster's Observations, during a Voyage round the World; where part of the crew, and some domestic animals, were poisoned by a particular species of fish. 'Sometime afterwards,' says the writer, 'I was told, that a fish of the same species was caught at Tanna by some of the sailors, who salted it and eat it, without any ill effects; whence it is to be supposed, that this species is not poisonous in itself, but only from the food, which it accidentally meets with,' &c.

These authorities, without mentioning others, are sufficient to establish the truth of the matter. Taking the fact, therefore, for granted, let us see what can be made of it. At first, we were inclined to look upon this as one of the strongest proofs that could be brought forward in support of the absorption of medicines; but, upon reflection, it appears to us, that it ought to be taken with considerable limitation, and that for the following reasons:

When fluids, or other matters in a state of solution, are enclosed in different cavities, they cannot, in a living state, be removed from those cavities, but by natural or artificial passages, or by absorption. The gall-bladder may be distended to a great size; the urinary bladder may be enlarged to an almost incredible extent, so as to be mistaken for dropsy; the liver may be filled with pus; yet, they never suffer any part of their contents to pass through into the abdomen. Indeed, if it were otherwise, it would not only endanger the life of the animal, but would altogether supercede the necessity of having reservoirs in the body for particular fluids. As soon however as death takes place, the case is altered. These reservoirs immediately lose the power of retaining their contents, and transudation is the consequence. This fact is very familiar to anatomists, who find the bile, the urine, &c. transuded, in almost all their dissections. Agreeable to this, it is easy to conceive, that poisonous matters, existing in the alimentary canal, may, after death, transude through and affect the muscles and other parts with their deleterious qualities; and might thus impose upon us the belief that they were absorbed during life. This is not speculation; but is rendered more than probable by the following words of Dr.

Thomas, the same sensible writer. whom we have quoted above. Speaking of the poisonous fish in the West-Indies, he remarks, 'when taken off the hook, if the precaution is used to gut and salt them *immediately*, they seldom or never create any disorder. The following facts evidently prove this. Mr. Henry Buckley, treasurer of the island of St. Kitts, is extremely partial to the barracuta (perca major of Brown) and never refuses to purchase them from fishermen whom he knows, provided they gut and salt them as soon as they are taken out of the water. He has never met with an accident since he adopted this practice, which is now upwards of thirty years.' Again: 'A fisherman caught some yellow-bill sprats in Halfmoon-bay, and, after using the above precautions of gutting, &c. threw the entrails into the sea, for fear of poisoning his favorite dog, which accompanied him in all his excursions: this happened in the morning. He carried the sprats home, and, together with his family, dined on them; in the afternoon he returned to his usual occupation of catching sprats, and observing his dog busily eating something, which it had picked up in the surf, he had the curiosity to examine what it was, and found it to be the guts of the fish thrown ashore by the waves. He immediately afterwards observed his dog in great agonies, and soon after he carried him home he died.' The above is related from unquestionable authority, and can be confirmed by the testimony of several of the most respectable inhabitants of St. Kitts. Another fact, equally important, happened to a Mr. Dupont at Palmetto-point. This man has been a fisherman more than forty years, and employs a number of negroes in drawing seines in different parts of the island. They one day caught a great quantity of yellow-bill sprats, which, as usual, he forbade his negroes to make use of, to avoid accidents; but, contrary to his orders, they gutted a number of them, and threw the guts on a dung-heap in an enclosed yard, where he kept his poultry. To his astonishment next morning he found a great number of them dead, but, on inquiry, none of his people were affected*.' Other instances of the same nature are related, which I omit.

* Memoirs of the Medical Society of London.

These facts are very important, and very much to our purpose. They prove, that the poisonous property of these fish is owing, as has been supposed, to their food; likewise, that they can be rendered innocent by taking out the stomach and intestines, *as soon as they are caught*. This circumstance, I apprehend is only to be explained upon the principle of transudation; for, if these poisonous matters, found in the stomach, &c. could be absorbed into the circulation, and afterwards diffused through every part of the body, it would be impossible, that the above, and improbable, that any other, precautions whatever, would be sufficient preventives against the effects of the poison. Whenever, therefore, any animal, otherwise wholesome, is found to poison, in consequence of a particular kind of food; I would attribute it to the cause already assigned; or else to carelessness or mismanagement on the part of the cook. This coincides with the intention of nature, who ordains, that the aliment of every animal should be decomposed; should be formed into chyle; in short, should become a part of the animal itself.

Does sulphur ever enter into the circulation? this has been answered in the affirmative by some, and their assertion is grounded on the following reasons. They say, that as 'sulphur, whether externally or internally used, produces a cure in the itch;' so, in either way, they presume, its operation to be similar.* In other words; when sulphur is taken internally, they suppose, it is absorbed, and, by means of the circulation, is applied, in its active state, to the seat of the disease on the surface of the body. Secondly, they assert, that 'sulphur tinges the silver, that may be worn by the patient of a black colour; and that it communicates its odour to the perspiration.'

These circumstances, if true, would no doubt, carry much weight; but, at present, we suspect that they carry more inaccuracy and deception with them than any thing else. For, that the internal use of sulphur will produce a cure in the itch, is a position not altogether so completely established as these gentlemen would make us believe. Many physicians deny it,

*Percival, Operations of Medicines.

and in particular we have the authority of Dr. Bonomo, who has paid much attention to the disease, and has thrown more light on the nature of it, than any other person. His words are, ‘ Neither do inward medicines perform any real service in this case, it being always necessary, after a tedious use of these, to have recourse to other external ones, already mentioned*.’ But granting, what is not probable, that sulphur, taken internally, is a cure for the complaint in question; even then the fact would not be a sufficient proof of its absorption. For sympathy, which is so active an agent in all the concerns of health and disease, would deservedly claim a large part of the credit. ‘ In cutaneous diseases we should remember, that the stomach may only be sympathetically affected; and that such disorders may be cured by the operation of medicines on the stomach†.’ That sulphur will tinge the silver, which may be about the patient, will not be denied; but the fact will be of no use to us in the present controversy, unless we could be certain (which is not the case) that the patients, in all such instances, were strictly confined to the internal use of the medicine. It is well known, that sulphur, in a state of vapour, is one of the most penetrating substances; so much so, that a small quantity, rubbed on any part of the body, will, by the application of heat, traverse every other. A person, merely by handling some of the sulphur-ointment, and afterwards going near a fire, had his sleeve-buttons rendered completely black. But independent of the foregoing objections, the absorption of sulphur is rendered very improbable by experiment. A gentleman in this city, whose talent for chemical research is very great, could not discover any sulphur in the blood of a patient whose system, to use a common phrase, was completely saturated with the medicine. I gave a man, a large quantity of the hepatic water, which, like other mineral waters, is said to pass off, by the urinary passages; but was not able, by means of the acetite of lead, to detect the presence of any sulphur in the urine.

* Philosophical Transactions Abridged, vol. 5,
† Jackson’s Medical Sympathy.

Among others, the presence of carbonic acid in the urine has been urged as a further proof of the absorption of medicines, and when this fact comes to us from so great an authority as Dr. Priestley, it is no wonder that so much stress has been laid upon it. It is, however, by far the weakest argument, that could be selected. When carbonic acid is united with other substances, it must exist either in a state of chemical combination, or in that of simple mixture. In the former case, the attraction is stronger, but in the latter, the acid is retained by a very slight force, and can be driven off by a low degree of heat; much lower, indeed, than the natural temperature of the human body. It is owing to this circumstance, that, when porter, cyder, &c. is taken into the stomach, the natural heat of the part produces a disengagement of the carbonic acid, and this is either belched up immediately, or else, is carried forward, and discharged per anum. This consideration renders it impossible that carbonic acid should ever exist in the blood, in a state of mixture; for, not only the heat, but the very agitation of that fluid, would be sufficient to expel it, and occasion death. Whenever fixed air exists in the urine, the same consideration makes it necessary, likewise, that it should be in a state of chemical combination with the earthly and alkaline bases, present in that excretion, otherwise, it would produce a flatulency, and consequently, a disease of the bladder. If the reader will only look at Dr. Priestley's experiment, he will find that this is perfectly correct; and that what the doctor took for carbonic acid was actually in a state of combination; for, 'it must be observed,' says he, 'that it required several hours to expel this air by heat; and after the process, there was a considerable sediment at the bottom of the vessel. This was, probably, some calcareous matter, with which the fixed air was united*,' &c. The urine of a person, who was constantly kept on the use of two or three bottles of porter a day, did not afford me any carbonic acid, when exposed to a heat sufficient to expel it, if it had existed there in the state of simple mixture. Let us then, for a moment, suppose, that Dr. Priestley,

* Priestley on Air.

to use his own words, has, 'more than once expelled, from a quantity of fresh made urine, by means of heat, about one fifth of its bulk of pure fixed air, as appeared, by its precipitating lime in lime-water, and being almost wholly absorbed by water*.' I ask, if this can be a sufficient proof of the matter in dispute, when we know, that an alkaline carbonate exists, naturally, in all urine? carbonic acid is secreted from almost every part of the body, and its presence in the urine is no more a proof of its absorption than the presence of the lythic, the phosphoric acids and ammoniac, &c. is an evidence, that these matters were absorbed.

The following experiment proves, that all urine will exhibit the same phenomena that occurred in the above mentioned experiment of Dr. Priestley.

EXPERIMENT.

A quantity of common urine was placed in an oil-flask and exposed to a boiling heat. The air that escaped was made to pass, by means of a syphon, through lime water. The first air that came over was nothing but the common atmospheric air of the vessel; but the next portion produced a large precipitation in the lime-water, and was greedily absorbed by water. Upon pushing the experiment a little further, the precipitate appeared to us to be in part re-dissolved, and there was, likewise, a very abundant white sediment, formed in the flask. At first, I had not the smallest doubt that it was the carbonic acid, which was given over, both in this, and in Dr. Priestley's experiment; but it struck me, that if it had been this acid, it never could have been driven from its combinations with lime and the fixed alkalines, by the boiling heat; nor did it appear to be united with ammoniac. I, therefore, began to suspect that it was not the carbonic acid; and I found my suspicion most handsomely confirmed by the next experiment.

* Priestley on Air.

EXPERIMENT.

A fresh quantity of the same urine with that used in the above experiment, was treated exactly as in the former case ; but as soon as I perceived, by the precipitation in the lime-water, that the proper air was coming over ; I removed the lime-water, and placed, in its stead, a clear solution of the acetite of lead. Here a copious white precipitate, immediately took place. This convinced me, that it was not carbonic acid ; for if it had been such,*no precipitation or change would have been made in the lead-water. As the muriatic, the phosphoric, and the lythic, acids are known to exist in urine, it must have been one of those salts.

We learn, then, that Dr. Priestley's experiment is correct, but that his deduction from it is wrong. The air, which came over in his experiment, certainly resembled fixed air, in precipitating lime-water, and in being absorbed by water ; and no person, without further experiments, could have possibly avoided the error into which this illustrious character has fallen. These experiments, though few, will, I flatter myself, be satisfactory to every person, and will be sufficient to set aside every thing that has been said concerning the absorption of carbonic acid. Lastly, it may be useful to mention, that the precipitate, produced in urine, by lime-water, is not, as has been advanced, any proof of the absorption of carbonic acid ; for all urine, that I have examined, will exhibit the same phenomenon.

We are told, by Mr. C. Darwin, that he discovered the presence of nitre in the urine of a friend, who had taken about two drachms of that salt *. As this experiment has excited a good deal of curiosity, and has contributed to confirm the belief of many in the absorption of active substances, to pass it by unnoticed, would, perhaps, be a mark of neglect ; I therefore, determined to repeat the experiment. But, before doing this, I found what I thought a satisfactory refutation of his pretended fact, in the following experiment.

* Zoonomia.

EXPERIMENT.

To a pint of urine was added a drachm of nitre, and after it had completely dissolved, I soaked a bit of paper in the solution, and afterwards allowed it to dry. Upon placing it in the flame of a candle, it did not shew the least appearance of nitre. I then mixed two drachms of nitre with the same quantity of urine, but was unable to discover this by similar means. I gradually increased the quantity of nitre, until I carried it as far as three drachms and a half to the pint of urine, and with this quantity I was but just able to detect it; perhaps, I should not have done it, even then, had I not been aware of its presence. The truth is, that Mr. Darwin's experiment was as hypothetical as the theory it was intended to support, and he deserves as little credit for the one as he does for the other.

'Fingere qui non visa potest ;.....

hunc tu,' medice, 'caveto.'

So far, then, as our experiments go, we have seen no reason for admitting the absorption of medicines. We proceed, in the last place, to answer, in a general way, some facts, which have been deemed very important, and no less decisive, against our opinion. One would be almost tempted, indeed, to believe in the absorption of active medicines, not only from a great body of plausible facts on the subject, but from the rage, that has always existed among physicians, for finding out another and a shorter passage to the bladder, than that of the general circulation. Their object was, to explain, by such a passage, the sudden appearance of the properties of those matters in the urine, that had been taken into the stomach; and it is, at least, reasonable to suppose, that these men were, in some measure, convinced of the reality of the fact, before they would set themselves to theorize about the probable cause of it. This circumstance, I may observe, would seem to be nearly as much in favor of absorption, as it is opposed to the notion of a new passage; for it is certain, on the one hand, that the properties of many substances do occasionally evince themselves in the urine; but although this new passage has existed so long in the imaginations of physi-

cians, yet it has, very unfortunately, never been found to exist in the body. Without wishing to avail ourselves of the advantage of this imaginary passage, I have mentioned it, merely to shew, that the qualities of bodies do exhibit themselves in the urine, and other secretions of the body; and as the fact is established by so much testimony, it must be admitted. Neither am I going to assert, that this fact has no weight at all. On the contrary, our senses, the only instruments of knowledge, are extremely limited in their operations, and, although it is a reflection humbling to our pride, it is nevertheless true, that they convey into the mind, not the essence, but only the qualities of matter. The qualities of substances are the only criteria by which we can judge of the presence of those substances. Indeed, it is perfectly impossible to disconnect the ideas of substance and quality, or to imagine that they can exist, the one without the other. Some of the properties of medicines, as smell, colour, &c. cannot, therefore, be supposed to exist in the secretions, without some material agent to produce them; and so far, the case is pretty clear. But, while we maintain this kind of language, we must not imagine, that the presence of one or two qualities, peculiar to any medicine, is an evidence of the presence of that medicine in its intire form. By a natural, though unfortunate, association of ideas, however, the matter has been otherwise considered. The whole has been judged of, from a part only; and opium, rhubarb, &c. are said to enter the circulation, because the odour of the one, and the colour of the other, are found in the secretions. Thus, while I am looking at a picture before me, containing the head of a great and good man, all his virtuous qualities are seen on, and are inseparable from, the picture; and I almost fancy myself in company with the great original. But it is well known, that all medicines, with which we are acquainted, are compound substances, possessing various sensible qualities, some of which reside in the one, some in the other, of their component parts. It is equally well known, that many, and for aught we know, the greatest number of these articles, are fitted to administer nourishment to the body. Thus opium contains a gum that is nourishing; stramonium is eaten by the goats and other do-

mestic animals; and the willow forms the bread of mankind, in some parts of the world; together with many other instances that might be mentioned. Nature knows no characteristic distinction between medicinal and esculent vegetables. They all appear to be composed of the same elementary principles, and the whole difference lies in the name. Medicines, therefore, like other aliment, when taken into the body, according to the power of the digestive organs, are decomposed. Some parts of them will be rejected as useless, others will go to the formation of chyle. These nutritive parts, it is presumed, will, even in the form of chyle, retain some of the sensible qualities which they possessed in the state of combination, and will thus be carried into the circulation. So that, whenever the qualities of medicines appear in the secretions, the only thing they prove is, that the nutritious parts of those medicines were absorbed. This may be illustrated by many facts and examples.

Thus sea-birds have their flesh sedgy, from living intirely on fish; yet, no one will believe, that the fish is taken up and swims, as fish, in the blood vessels. A circumstance, much in point, is related by Dr. Percival. ‘Extract of logwood, taken internally,’ says this ingenious physician, ‘sometimes gives a bloody hue to the urine. But the astringency of it does not, according to my trials, accompany its colouring matter*.’ In like manner, certain articles of food, as the different species of the siliquosa, the asparagus, the garlic, the indian fig, &c. all communicate their smell or colour to the urine; yet, do we not know that these vegetables nourish the body, and, to answer that purpose, do we not know that they must be formed into chyle? Madder communicates its colour to the bones, &c. yet, do we not know, that madder contains principles that are nutritious? The turkey-buzzard feeds on putrid animal matter, and it is said, that this is absorbed as putrid matter, because, forsooth, the feathers of these birds have a stinking odour! Now, when we reflect, that these birds are always up to their eyes in putrid matter, and always surrounded by a putrid atmosphere; when we reflect, that

* Manchester Memoirs.

putrid matter, according to Spallanzani's experiment, will be rendered sweet by the action of the gastric juice upon it, and when we reflect, that this putrid flesh is not only nourishing, but that an animal will be killed, if only one drachm of putrid serum be injected into its vessels, we cannot but be surprised, that a fact of this nature should be looked upon as a proof of absorption. Let the reader extend this principle, and he will be able to explain many other facts.

We have thus considered the most interesting particulars in favor of the absorption of medicines, and shall conclude, with this observation to the reader: *si quid novisti rectius istis*; which I do not doubt, *candidus imperti: si non, his utere mecum.*

AN INAUGURAL DISSERTATION,

IN WHICH, BY AN INDUCTION OF FACTS FROM

DYSENTERY,

THE

MITCHILLIAN DOCTRINE OF PESTILENTIAL
FLUIDS IS ILLUSTRATED.

SUBMITTED, TO THE PUBLIC EXAMINATION OF THE

FACULTY OF PHYSIC,

UNDER THE AUTHORITY OF THE TRUSTEES OF COLUMBIA
COLLEGE, IN THE STATE OF NEW-YORK,

The Right Revd. BENJAMIN MOORE, D. D. President;

FOR THE DEGREE OF

DOCTOR OF PHYSIC,

ON THE NINTH DAY OF NOVEMBER, ONE THOUSAND EIGHT
HUNDRED AND TWO.

BY NICHOLAS I. QUACKENBOS, A. B.

OF THE STATE OF NEW-YORK.

AN
INAUGURAL DISSERTATION
ON DYSENTERY.

CHAP. I.

LITERARY AND PHILOLOGICAL REMARKS.

A great controversy exists among Physicians concerning the true meaning of the words *contagion* and *infection*. Some have considered them as synonymous, and others have contended that they signify things of a very different nature. It does not, perhaps, become one just entering into the profession to decide on this point, upon which men of high character and eminence have differed so widely; yet I cannot suppress a belief that the two words did originally denote ideas very unlike each other, and that at this time they ought not to be confounded.

There is scarcely an instance of two words in the English, Latin, or any other language, possessing the same critical meaning. Though in common speech, they may be employed as convertible terms; yet they are always found, on nice examination, to have a plain and sensible difference. The books of rhetoric and belles lettres inform us wherein 'pride' differs from 'vanity;' how 'fatigue' is distinguished from 'uneasiness;' and by what means 'delight' varies from 'pleasure;' with hundreds of other examples of the kind. These, though in common acceptation, reputed to be synonymous words, are in reality very far from each other in true signification.

What happens in the language of common life occurs also in the dialect of medicine. Words reputed by many to be

quite alike, are known by the correct and learned to intend things widely remote in their meaning. Thus 'Lues,' 'Pestis,' 'Contagium,' and 'Infectio,' have been supposed by many as words of signification so nearly alike, that in glossaries and lexicons they have been employed familiarly one instead of the other. But I shall endeavour to shew that this is a mistake.

1. 'Lues' seems to be derived from *luo*, to pay the cost, make atonement, or suffer punishment for a crime or fault. Hence *luere penas* signifies to suffer the penalty for an omission or breach of duty. And, for the same reason, 'lues' is employed to mean any distemper brought on through or by a violation of moral obligation: particularly it applies to the disease consequent upon scortatory love, which has been termed emphatically 'Lues Veneria,' the malady incident to prostituted embraces. Such is the literal and original meaning of the word; but like other words, it acquired afterwards a greater latitude of signification.

Thus Claudian the poet writes.

'Hinc hominum, pecudumque lues, hinc pestifer aer;'

alluding to the sufferings of men and cattle; and Virgil goes a step further, and extends the idea to trees and corn:

'Arboribusque satisque lues, et lethifer annus.'

2. Whether 'Pestis' is derived from 'perco,' to perish or to die; or comes from the Hebrew 'paschat,' importing to spoil or pillage, seems not necessary now to be disputed. It is sufficient for the present purpose to show that whether the former or latter etymology be adopted, *pestis* means something ruinous and destructive; generally applying to such calamities as cannot be prevented by human foresight. Pestilence is therefore coupled by Virgil with the anger of the Gods:

'Pestis et ira Deorum stygiis sese extulit undis.'

And again there is classical authority for the following:

'Me eruciat sævo pestis violenta veneno;'

showing that the word originally signified almost any unavoidable distress; and, in a limited sense, applied particularly to disastrous sickness, and endemic distempers.

3. No person doubts that *contages*, *contagium*, and *contagio*, are fair derivatives from *contingo*, to touch or be in contact. Their primitive sense doubtless was, 'Diseases communicable by approximation of skin to skin.' Hence these forms of expression apply peculiarly to *gregarious* animals, as sheep, cattle, and the human species. Creatures of these kinds, herding and mingling together, associating while they feed, and when they lie down to rest, are remarkably prone to catch diseases by *contact*.

In this strain Melibæus assures Tetyrus that (Virgil, Ecl. i. v. 51.) the noxious contagion of the neighbour's flock shall do no injury to his :

'Nec mala vicini pecoris *contagia* lædent.'

The forms of expression, when either of these words is used, are adapted to give an idea of something creeping, or passing off from one person to another—thus :

'Dira per incautum *serpunt* *contagia* vulgus.'

And again :

—————'Dira per omnes

Manabant *populos* *fædi* *contagia* *morbi*.'

And also :

'*Invadunt* *totum* *contagia* *morbida* *regnum*.'

From these authorities it would seem plain, that the popular and obvious meaning of *contagion* could scarcely be misunderstood, since it respected merely that class of disorders among men and brutes, which were imparted from one to another by *contact*, as they fed, slept, played, and associated together.

Contagion therefore is that peculiar, morbid poison, which is prepared in the bodies of living animals, especially of those which flock and huddle in crowds, and is wiped off, or is communicated by touch from those that are contaminated, to those which too nearly approach them. Such was the common sense of mankind, while it remained unwarped by prejudice and forced meanings. It is a pity that such unlettered, though strong common sense was ever departed from.

4. 'Infection' was certainly derived from *inficio*. This word was of very various and questionable meaning among the Romans. It is compounded of *in*, negative, and *facio*, to do,

signifying to *undo*, or rather to violate, to corrupt, to taint, or to tincture any thing. To be a little more particular—the verb *inficio*, expressed that property of an agent, by which it embued the substance upon which it acted, manifestly and glaringly changed its qualities, and altered it materially from what it originally was. The substance or thing so altered was said to be *infected*.

One of the most frequent and distinguishable cases of *infection* was the change which white cloths underwent by dying. The colouring material, or the dye-stuff, was the agent, and the changing or vitiating the white colour by its tinging property, was said to *infect* or *stain* the fabric; its whiteness was alleged to be *overcast*, *polluted*, *spoiled* or *undone*.

So when the *clear* and *fine* atmosphere was vitiated by mixtures of *noxious vapours*, and *poisonous gases* extricated from corrupting bodies, it was declared to be *infected*. The pure air, like the white cloth, had acquired a foreign tincture; and this effect, wrought upon the respirable and healthy atmosphere by the adventitious material with which it was charged, was, in a figurative sense, denominated *infection*. The air so vitiated or corrupted, so different in its constitution, and so altered from what it was, literally speaking, had become *infected*, or ‘*infectus*,’ that is, *undone*, or *spoiled* for the purpose of its primitive and ordinary destination. Air thus vitiated was *infected air*.

In like manner, a healthy animal might be *infected* by coming into an atmosphere of such a tincture, or so corrupted. An external agent of this kind could infect or undo the healthy frame. When this act of *undoing*, or, in other words, of *unfitting it for the performance of its accustomed and useful functions*, was accomplished, the constitution was affirmed to be *infected*: as was observed in a preceding paragraph, infection was, in one of its senses, but another term for dying or imparting colours. In many forms of distempers excited by pestilential air, there was observed, in addition to the other symptoms of debility, &c. a remarkable change of complexion. In many cases the patients looked almost as if they had been dyed, or coloured with some tinging material. This confirmed the notion of infection having penetrated the body, and wrought a

change, as evident to the eyes of others as uncomfortable to the feelings of the individual himself. And by this process of the human mind, it seems to have been accepted and understood, that a white garment put into dye-stuff, pure air exposed to septic exhalations, and a healthy animal acted upon by a pestilential atmosphere, were all examples of '*infection*.'

The term was applied to another case. When any thing noxious was added to wholesome drink, it was declared to be *infected*, that is, to be undone or spoiled for the natural and intended purpose of slaking thirst healthily. The beverage was infected; *i. e.* the pure liquor has had something infused into it, by which it has been vitiated or poisoned. Hence the verse can be interpreted:

'Pocula si quando sævæ *infecere* novercæ.'

Having thus discussed the history of these words, I shall observe that they all refer to popular and not to scientific distinctions in things. They may perhaps answer well enough for the purposes of ordinary conversation, but possess not sufficient distinctness for those of medical and philosophical language.

Considering the matter with the best information I have had, there appear to be but two great operations in nature, for preparing or engendering noxious fluids. The former of these is accomplished by the instrumentality of the living vascular action of animals, and it may be, of vegetables. The latter is produced by a putrefactive process taking place in certain organized bodies after death. To signify these two grand natural processes, there ought to be invented two suitable and appropriate terms. But as such an innovation in the technology of the profession might not be right, I shall employ the two words *contagion* and *infection*, to denote them.

When noxious fluids are produced by living vascular action, I call them *contagion*: when they are the offspring of putrefaction, I term *infection*.

But as this subject has been stated with great perspicuity in that valuable periodical work, the Medical Repository; a work which is an ornament and an honour to our nation, as well as our city; I shall quote the passage (vol. v. p. 186.)

‘ *That vitiated product of living vascular action, which can excite in a well person a disease like that by which itself was produced, and continue indefinitely to do so after being transferred from one body to another, will be denominated contagion; and lues, vaccinia, measles, and small-pox, will be considered examples of it. On the other hand, that venomous offspring of putrefaction going on in some of the kinds of organic matter after death, or separation from the living frame, which disorders the healthy functions without being specifically communicable, and without the power of communicating itself, will be called infection; and typhus, dysentery, plague, and yellow-fever will be given as instances.*’

CHAP. II.

MEDICAL AND PHYSIOLOGICAL CONSIDERATIONS.

WE now proceed to the *medical* history of our subject. In describing this, we shall depart from the common mode of mentioning the symptoms, diagnosis, &c. and offer such reflections as have occurred.

It appears evident that the combination of elementary substances, which, when applied *externally*, would occasion the disorders commonly called ‘fevers,’ would do the same, if they came in *contact with the internal parts*. This is well shewn in a piece published first in the New-York Magazine, in 1797, and afterwards in the Medical Repository for that year. The principal points which appeared at that time, were, that septic acid frequently existed in the alimentary canal as an exciting cause of fevers, and that neutral salts were more useful than other cathartics in such cases, because a great proportion of them could neutralize *that acid* by means of the superior attraction existing between it and their alkaline bases.

Doctor William Bay, who graduated in physic, in May, 1797, in this college, chose *Dysentery* as the subject of his Inaugural Dissertation; and having adopted the above princi-

ple, endeavoured to show that if septic acid was the exciting cause, *neutral salts, in which pot-ash and soda are united to the weaker acids*, were very efficacious remedies. This dissertation fell into the hands of the monthly reviewers in London, and the writer of the criticism upon it rejected the doctrines contained in it both practical and theoretical. And more recently, in one of the numbers of the Monthly Review, for 1801, the writer of the criticism upon Dr. Chisholm's Essay on Malignant and Pestilential Fever, speaks disrespectfully of that excellent work, because the author has advanced doctrines derived from the same source.*

Anatomists have agreed that the cuticle reflected or continued over the lips at the mouth, passes down the pharynx and œsophagus, and after lining the stomach and intestines, both small and great, is connected again with the true skin at the verge of the anus. The inside of the body then, or what is called the alimentary canal, in all its turnings and windings, is a surface coated with an epidermis in connection with the muscular, vascular and nervous parts beneath, *without the intervention of a true skin*. It resembles, in this respect, cicatrization on the limbs, or other external parts of the body where spots that had been ulcerated, are skinned over af-

* Vide the note. page 14 of Chisholm's 2d volume.

‘ See the case of the manufacturers of soap and candles in the city of New-York, stated and examined, &c. published by the association of tallow-chandlers and soap-makers.’ The advocate employed on this occasion was Dr. Mitchell, the ingenious and learned professors of Chemistry in Columbia College; and the scientific knowledge, the general erudition, the good sense, and the elegant language displayed in the course of his argument, in support of his clients, must secure the admiration and applause of those who read his Remarks on the Proceedings of the Legislature of the state of New-York;’ and his ‘ application of the *Mitchillian Doctrine* of septic fluids, to the processes carried on in several branches of handicraft business, particularly the making of soap and candles,’ &c. Without a hyperbole, he may ‘ be considered,’ to use his own language, ‘ as having caught nature in her work-shop, examined her collection of raw materials, and discovered which of them she employed in this fearful manufacture (the acid of putrefaction or infection,) which, like the poisoned shirt of Hercules, enwraps the wearer too closely to be shaken off.’

p. 53.

ter a destruction of a portion of the *cutis vera*. Such places are tender, and more liable than others to accidents.

We call the stomach and intestines a part of the *internal structure* of an animal. There is one sense, in which this is hardly correct, or is apt to mislead. Their internal structure is as much an inside as the barrel or bore of a *pump* is the inside of that machine. They are open at both extremities, the substances they transmit are forced in at one end, and expelled at the other, or at some intervening outlet. Air and water move easily through, and are applied freely to their insides as well as to their outsides: and besides those two fluids, a variety of substances mingled with them, even to the thickest consistence that can pass the valves, are pushed along their cavities. The bore of a pump is thus, strictly speaking, an outside surface, or at least as commonly and indeed more exposed than that which is ordinarily termed the outside. The perforation through the body of an animal, extending from the mouth to the anus, ought to be deemed an *external* surface, or, at least, a surface as much or more exposed to harm and accident, than any part of the external or cuticular surface. It is the thoroughfare of every thing put into it for the purposes of hunger, thirst, gluttony, intemperance, and medicine. The alimentary canal being thus, like the pump, in almost constant action, and besides the perpetual conduct of foul and noxious matters, will often get out of order, and of course stand in need of frequent repairs.

One of the accidents to which this internal apparatus is frequently subjected, is called '*Dysentery*,' and has been known by a synonymous word from a remote antiquity.

Such persons as subsist chiefly on oily, gelatinous, farinaceous, and saccharine food, are very little incommoded by this disease. On the other hand, the victims of its violence are commonly such as eat animal food heartily, particularly that which is *lean*. In the American and British service, both by sea and land, *dysentery* is nearly connected with the *beef-ration*, which is dealt out to the seamen and soldiers. Our experience in New-York has convinced us of the readiness of *beef* to corrupt, and of the offensiveness and virulence of the gases which exhale from it during that process. We know these

vapours have very often produced *dysentery* in the repackers and salters of beef, who were exposed to it. And from a variety of testimony it may be concluded, that this deleterious effluvium from *beef* is septic acid (the acid of azote) in a volatile form. Of a number of persons exposed to this septic effluvium, some were seized with *dysentery* and others with *fever*. Such a disposition and quality we know to reside in *beef*, which I have given as an example, and doubtless inhere in all other *lean* animal substances.

Beef, in a corrupting condition, called tainted or semi-putrid, is not an uncommon article of diet in armies and navies. Whether this vitiation arises from the small quantity of muriate of soda (sea salt) or from an adulteration of the muriate of soda applied, or from an inherent predisposition in the beef to spoil, the ultimate effect is nearly the same. It becomes charged with that *acid*, which is capable of producing those mischievous effects in the alimentary canal, the aggregate of which is denominated *Dysentery*. Or, as the septic poison in the meat acts upon a secreting surface, it may cause a *flux* in some constitutions, as well as a *dysentery* in others, and act as a cause of some species of *diarrhæa*. If absorbed, it stimulates the sanguiferous system to morbid action, and induces that condition thereof called '*Fever*.' Being of an acid quality, it irritates the orifice of the common bile-duct, and provokes a more free secretion and supply of the gall. This *alkaline* fluid from the liver, in moderate forms of disease, is generally excreted copiously, and thus neutralizes the exciting cause. As the bile is plentifully prepared and discharged, the diseases in which it abounds are commonly called '*Bilious*;' and the ordinary opinion is, that the bile itself is the peccant matter or cause of the malady. But this notion is very erroneous. The bile is but the *effect* and not the *cause*. And the reason wherefore it runs so readily and plentifully, is to correct the mischievous quality of the offending cause in the stomach and intestines, whether engendered there or taken in from without: accordingly, some of the worst forms of *dysentery*, and other malignant distempers, are those in which the bile is excreted from its viscous in scanty proportion, or not at all.

Though the alimentary canal may receive the septic acid

or exciting cause of *dysentery* from the atmosphere, or in tainted beef or other *lean* animal substance, these are not the only ways by which it may come in contact with the stomach and intestines. Lean animal matter, as beef, mutton or fish, may be eaten in its entire and uncorrupted state ; but it may be so long retained in the body, especially in the large intestines, that it may corrupt, and septic acid be formed from it there ; and this accident is very liable to befall those persons who are subject to indigestion and costiveness, or, in other words, who are not regular in respect to alvine evacuations. In such cases scybala may be formed in consequence of too great absorption of the fluids of the intestines, and from the residuary lumps of hardened fæces, containing *some* septic acid, and engendering *more*, is the spasmodic condition of the colon induced.

A cause of dysentery may thus be received into the body from without, or it may be produced within it from the decomposition of such articles of food as contain septon or azote. The former is the distemper deemed *infectious* or *endemic* ; the latter, the *sporadic*. But it may so happen on board a ship or in a camp, that many persons of a crew, or a detachment fed on *similar rations*, may fall sick together from the unwholesome quality of their food, without any vitiation in the air at all ; and the disease, though strictly *sporadic*, may thus assume the guise of an *endemic*. The like may happen in neighbourhoods and districts in the country, where the inhabitants live very much alike, or subsist on nearly similar articles of diet, and feed on meat cured with muriate of soda (generally adulterated, and rarely or never purified in America,) bought at the same store, and part of the same parcel.

If there should happen a condition of the digestive organs, wherein the gall should cease to flow at a time when *oxygenated septon* from any of the sources already mentioned, abound in the alimentary canal, disease might be expected to ensue. But such a disorder would not be a *mere jaundice*, from a retention of bilious matter tinging the skin with an icteritious or yellow hue, but would be a disease *from poison*, causing the stomach and upper intestines to invert their motions and expel their contents ; creating pain, flatulency, heat, and inward distress ; and stirring up more or less of '*fever*' by an imbibition

tion of some of the *septic venom*: to all which there might be superadded the peculiar and characteristic symptoms of *dysentery*. For violent fits of the American yellow fever have occurred in cases where the stomach and intestines had been much disordered, terminating favourably after tormina, tenesmus, and slimy stools mixed with blood. Whence the connection between *yellow fever* and *dysentery* can be discerned; *both* arising from the same general cause, and chiefly differing in the part of the body particularly invaded by that cause. Dysentery may exist without being accompanied with yellow fever, and yellow fever may arise unattended by bloody-flux. Yet though it may be easy to distinguish the extremes, there is nothing more difficult than the establishment of the limits between the two, in cases where they are blended and incorporated with each other. In short, though *Nosology* may distinguish, and sever and place them far asunder, *science*, with better information, traces their genealogy from one original, and finds them intimate kindred, descended from the same parent.

This is by no means a new opinion. If we look back into history, we will find that the ancients were also well acquainted with this fact. Diodorus Siculus, who has always been considered a correct and faithful historian, (lib. xii. cap. 2.) gives an account of the expedition of the Athenians under Eurymedon and Demosthenes against Syracuse. This was about the 410th year before the Christian æra. A *plague* broke out in their camp adjoining the city, owing to an *offensive marsh* in the neighbourhood, and raged and increased to such a degree, that a great part of the army perished.

Diodorus (lib. xiv. cap. 7.) describes, with some detail, the distemper which invaded the Carthaginians in the year before Christ 394, when encamped on the same ground, near Syracuse, that had been formerly occupied by the Athenians. He calls it a 'plague;' says it was first ascribed to the vengeance of the gods, for the rifling and plundering of the Temples of Ceres and Proserpine. But he considers the *place itself* as the great occasion of the disorder. The ground was marshy and spongy—great multitudes of men were thronged together—it was summer—the nights were chilly and the days intolerable.

bly hot—the pestilential air was blown upon them by a southerly wind—there was no idea of importation from foreign places—the symptoms were *catarrhs* and *swellings* of the *throat*, which were caused by the *stench* of the bodies that lay unburied, and the *putrefaction* of the soil. Then followed *fevers*, *pains in the back*, *heaviness of the loins*, *dysenteries*, *blotches* and *boils* over the whole body. Some ran mad, beating every one they met.’

The exciting cause of dysentery being known, there is no difficulty in administering remedies which, in most cases, will destroy or expel it. From the known efficacy of alkaline salts to correct putrid taints and tendencies in beef, and in all animal substances, *pot-ash* and *soda* present themselves first on the list of anti-dysenteric remedies. We know they can correct offensive and virulent qualities of the *fæces out of the body*, or in beef after it has been eaten, and naturally enough can believe they will act in a similar way *in the intestines*, provided they can by any means be conveyed there.

Notwithstanding the prejudices of some against the employment of *alkalies*, and the belief of others in their being unwholesome and improper in the extreme; that *acids* are the great and useful antiseptics, and of course, by the rule of opposites, that *alkalies* are endowed with qualities directly the reverse: notwithstanding all this, I am one of those who have an entire confidence in the superior excellency of *alkaline* medicines.

It is known that in some parts of our country the persons who practise the veterinary art, give to horses, and other creatures, *weak alkaline ley*, made by boiling wood ashes in water, for the colic and scowers. And occasionally the country people will, in similar diseases, take some of the like medicine internally for their own relief. Besides, it is said that during a severe dysentery which prevailed among the Indians near Detroit, some years ago, the prescribers of medicine among the savages administered weak ley of pot-ash to the sick, and with admirable success.

A strong solution of muriate of soda, diluted with a third of sharp acetic acid, has been often administered to dysenteric

patients, as is credibly reported, with excellent effect. After recommending this remedy, and witnessing the operation of the brine, swallowed as hot as the patient could bear, in doses of two or three table spoons full, repeated frequently, I have had reason to think favourably of it, as an antydysenteric prescription. And what seems more capable of being deduced in favour of alkalies in the intestinal canal, is the known constitution of the gall; a secreted fluid abounding with the very alkali which is the basis of common salt, and in times of health perpetually mingling with the alimentary mass, for the purpose apparently of preventing its degeneracy to something noxious. It seems therefore clear, that a solution of carbonate of soda in water might be employed at any time in aid of the bile, or in some measure as a substitute for it if deficient, and thus allay the uneasiness and pain frequently caused by it.

We know that gall has been long employed by dyers and scourers to cleanse silks and delicate stuffs, and to free them from spots and stains; it is therefore not improbable that this saponaceous liquid acts in a like manner in the alimentary canal, deterging and cleansing the whole passage, as far as its strength and virtues extend.

Alkalies promise to be useful on another account. The discharge per anum in some of our pestilential and dysenteric diseases, are so sharp and corrosive as to excoriate the skin around the anus, and to excite inflammation almost wherever they touch an external part.* When shirts, sheets, or drawers are besmeared with these or any other kinds of fecal mat-

* In Lowthorp's Abridgment (Phil. Trans. vol. iii. p. 232.) may be seen an account of Mr. Bayle's Experiment related to the Royal Society in 1664, of the antiseptic power of fine urinous spirit, or spirit of sal ammoniac, in preserving blood yet warm from the veins, from coagulation and from *putrefaction* a long time. This experiment, he says, he devised to show the 'amicableness of volatile spirits with the blood.'

In the same volume (page 115) in a paper on dysentery, may be seen the writer's opinion that the sharp corrosive humours in that disease, 'are of the nature of *aqua fortis* and *spirit of nitre*, and which eat away the tunics of the intestines and mouths of the vessels.' For correcting this he adds 'the absorbent earth crabs-eyes to his prescription.'

matter, alkaline leys are always competent to destroy their activity, to carry them off, and to leave the garment clean and uninfected in the hands of the washer.

If alkalies can accomplish these desirable objects in alimentary *egesta* immediately after their evacuation, there remains no doubt of their capability to accomplish as good a purpose, if injected into the *intestinum rectum*, and made to penetrate the colon itself.

In addition to all this, it seems to be established as a truth, that in those parts of the United States where carbonate of lime constitutes the great strata of the earth, and where of course the water contains a considerable quantity of lime in solution, dysenteric complaints are comparatively rare and mild. The lime-water which the inhabitants of these situations constantly drink, acting always as a *preventive* of the disease, or as a *corrector* of its exciting cause within the body.

We shall next consider which of the alkalies will be preferable, and also what form or combination of either will answer best the purpose of prescription.

To give the alkalies in their simple or caustic condition would be injurious: the disease would be aggravated, not cured by such harsh medicines.

I begin with pot-ash (salt of tartar) and would give the carbonate in dysentery, making a solution of it of such a strength as, on being tasted, should not affect unpleasantly the mouth. To this, sugar may sometimes be added to help the taste. A table spoonful may be given every quarter or half hour, according to circumstances, until the patient experience relief. And in order to render its effects more speedy and certain, enemata of the same weak alkaline solution should be given from time to time, either alone or with broth or starch; occasionally with the addition of a small quantity of thebaic tincture to the mixture taken by the mouth, as well as to that administered per anum. Much benefit will, I presume, be derived from this mode of treatment.

On trials with carbonate of magnesia, it appears to be not sufficiently strong and efficacious. Its powers are greatly inferior to carbonate of pot-ash. Though its qualities are good

as far as they go, yet they are, however, too feeble to be relied on in cases where decisive and energetic practice is required.

The disagreeable taste of pot-ash is sometimes objected to by patients. When this happens, carbonate of *soda* may be employed. This is much less unpleasant, and may be tasted and swallowed with ease: indeed, on reflecting that *soda* is the *basis of the bile*, and of the *culinary salt*, which we constantly and by instinct as it were, swallow with our food, it appears to be more natural and friendly to the constitution than either pot-ash or magnesia; and for this reason it is that neutral salts, with a basis of *soda*, are preferable to all others, for their efficacious, safe and kind operation. The bringing them more generally into use would be a great improvement in the practice of physic.

The formula of giving carbonate of *soda* in dysenteric cases is, a solution in water strong enough to be taken into the mouth and stomach without smarting, or any other inconvenience. The dose is from a tea spoonful to two or three table spoonful; and the times of administration are every quarter or half hour, or every second and third hour, as the symptoms seem to require. Portions of the same solution are applied in the form of glisters, to be retained; and they are very useful and efficacious in allaying tenesmus, and diminishing the frequency of evacuations. I have seen several cases of dysentery cured with this plain prescription in the *two* modes just mentioned.

Sometimes, however, the carbonate of *soda* may be dissolved in mint-water, instead of common water: and occasionally the addition of some laudanum, where the patient suffers much pain, has had a happy effect. I think where alkalies are prescribed there need be less hesitation to administer opiates than in conjunction with any other remedy. Doses of *ol. ricini*, or of *ol. olivarum*, may be occasionally interposed with advantage, as mild oils have a tendency to obtund the acid exciting cause of the malady. The same quality probably belongs to all fat and greasy substances that are not rancid. On the contrary, *lean* meats are found universally bad for dysenteric patients, who ought always to be interdicted their use. I have known a convalescent, relapse into dysentery after eat-

ing a *dinner of beef*. For food we would recommend *rice*, *tapioca*, *sago*, *panado*, and generally *farinaceous* and *saccharine* substances; and likewise soups and jellies, if the sick have an inclination for them.

In all these kinds of food an advantage would be derived from sprinkling in as much *muriate of soda* as can be conveniently and agreeably done.

By these means the alimentary canal can in general be sufficiently alkalized. We alkalize our clothes and the outer surface of our bodies with solutions of soap and weak leys, to keep ourselves clean and healthy. The intestines are alkalized by the *bile*, and their internal surface is protected by the mediation of that admirable liquid, which is prepared and applied without our knowledge or concurrence, by the most excellent provision in the animal economy. When the gall is insufficient, it becomes the physician to alkalize the intestines and their contents, by something as nearly allied to the bile as he can find. *Soda* is such a substance. Perhaps camomile, gentian, or some other bitter, would improve it, by making it resemble the natural secretion more nearly.

The *carbonates* of pot-ash and soda seem to be better in dysentery than the combination of those simple salts with stronger acids. They are more easily decomposed, and while the septic acid of the intestines joins the alkalies, to form *septics* of pot-ash and soda, the carbonic acid is extricated to produce the agreeable effect of which it is supposed capable, and for which it has been long celebrated. But it ought to be remembered that the *tartrites* of *pot-ash* (soluble tartar) and of *soda*, (Rochelle salt) and *phosphate of soda*, by which is meant the compound formed by mere muriatic acid and soda, and not the adulterated and heterogeneous composition forming the sea-salt commonly in use. No objection ought to arise against the use of these alkaline remedies, in ordinary cases, as being too fiery and pungent; nor even in cases of inflammation and ulceration of the intestines: for it is shown, in a memoir in the Medical Repository, by Dr. Mitchell, that carbonates of pot-ash and soda are very substantial helps to the surgeon, when applied to the surface of foul and eroding ulcers. And from their internal use, another

good effect will ensue : the fæces will possess very little fætor, and no infection. Nurses and attendants may perform their services commodiously, and without hazard of *catching* the distemper. Under the use of these remedies it can never spread.

Alkaline remedies, excellent as they are, may be abused ; and for want of proper care in their administration and continuance the customary good effects may not be produced by them : but they can do a great deal ; *for they can overcome the acid exciting cause of the dysentery, and prevent its further mischievous operation upon the intestines, and its absorption into the system.* They may do all this, and yet not be capable of curing every case that presents.

Inveterate dysenteries and fluxes will often baffle medical skill, and end fatally, in opposition to alkaline and all other remedies. After the stomach has lost, in a considerable degree, its power to digest, the liver to furnish bile, the lacteals to imbibe their appropriate fluid, and the intestines to perform their peristaltic movements, there is no great prospect of recovery, even though the exciting cause of the malady should have been entirely expelled from the body. Still it appears, from repeated trials and careful observation, that the alkaline plan of treatment which we have described, is preferable to every other. Rhubarb, ipecacuanha, cerated glass of antimony, calomel, and Peruvian bark, seem to be very inferior remedies. None of them promise to do much good, further than as they operate as cathartics. And for a purgative purpose, the neutral salts which we have enumerated possess a decided superiority.

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AN INAUGURAL DISSERTATION,

ON

THE EFFICACY

OF CERTAIN

EXTERNAL APPLICATIONS,

SUBMITTED TO THE EXAMINATION OF THE

REVEREND JOHN EWING, S. T. P. PROVOST;

THE TRUSTEES AND MEDICAL FACULTY OF THE UNIVERSITY
OF PENNSYLVANIA,

FOR THE DEGREE OF DOCTOR OF MEDICINE;

ON THE TWENTY-SEVENTH DAY OF MAY, A. D. ONE THOUSAND
EIGHT HUNDRED AND TWO.

BY HENRY JACKSON, OF SAVANNAH, GEORGIA,

HONORARY MEMBER OF THE PHILADELPHIA MEDICAL AND
CHEMICAL SOCIETIES.

'Non recito cuiquam, nisi amicis, idque coactus,'

Hor.

INAUGURAL DISSERTATION.

THE period of the first application of medicines, to the external surface of the body, must have been very remote. The practice is, probably, to be ranked among the first attempts that were made, in the early infancy of our science, towards the removal of disease. At a time when chemistry had not yet discovered the different metallic preparations, or experience ascertained the nature and dose of the active vegetables, medical prescriptions were necessarily confined to external formulæ. Accident may have first suggested their utility. The common incidents of life must have afterwards established it. The relief afforded in febrile head-ach, by the application of cold water, or of a cool green leaf; the alleviation of pain in other parts of the body by means of warm fomentations, the gentle friction of the hand, emollients, sinapisms, and blisters, must all have tended to the confirmation and continuance of the practice. Accordingly, if we consult the *materia medica* of those people who have made but little improvement in the medical art, we shall find it to be composed chiefly of such articles, as relate to external application; such are the hot and cold bath, frictions, the potential caustic, and fomentations of different herbs. The majority of these items are, at present, among the principal remedies of the Indians of North-America.

In proportion, however, as the number of our medicines has increased, by the assistance of chemistry, natural history, and repeated experience, the manner of administering them has undergone a material change; and, at the present day, external applications, by being limited to local affections, to certain parts of the body, or to certain stages of a particular disease, may be said to be almost deserted. Whether or not this change has been justified by their deficiency of effect, or a want of success in their use, the following observations and experiments, may, perhaps, tend to show,

THE practice of applying medicines externally, is founded on the intimate connection of the skin, or external surface of the body, with every part of the system. This connection, on whatever it may depend, is illustrated by many familiar phenomena of life, both in health and disease. The languor and debility experienced on a hot summer day, the hilarity of mind, and activity of body occasioned by the coolness of a subsequent evening, are instances of it; as well as the very sensible effects produced on the system, in regard to appetite, activity, gaiety, or the exercise of body or mind, by the sudden changes of our variable climate, in every season of the year. On this connection depends, also, that pleasing, though indescribable sensation, which every one must have experienced from a change of linen and fresh garments—a species of self-feeling, the gratification of which, forms, perhaps, the only justifiable luxury, as its indulgence, however far carried, can tend only to the preservation of health. In some persons we find the effects of this change to be very remarkable: an instance of which is mentioned by Dr. Rush, in a gentleman to the southward, who has always recourse to it with effect, in order to remove an approaching fit of hypochondriacism. A flannel shirt will excite, in a person unused to its irritation, a slight degree of fever; and, on the contrary, a departure from the usual quantity of dress, or of bed-covering, will

occasion a catarrh, or a pleurisy. So intimate, in fact, is this connection, that an unusual, though apparently a very innocent impression, will produce sometimes a very alarming disease; in proof of which, there are instances of persons, during the late war, who were seized with convulsions, the first night they exchanged the earthen floor for the feather bed.

In disease, we find this connection to be equally, if not more strongly manifested. The powerful effects of cold applications to the skin, in hæmorrhagy, the varying state of the skin itself, according to the state of the disease; and its affection as a premonitory symptom of disease, all evidence it. Hence the reason of its having been considered by physicians, as early as the first annals of medicine, as of the first importance in forming a prognostic of the favorable or unfavorable issue of any general affection.

Do not all these circumstances, whilst they demonstrate the connection of the skin with the system, point out the propriety and utility of external applications? Another observation cannot but tend to strengthen this inference—it is, the extension of this connection in a state of health, to the mental part of our frame, by the common observation of mankind, which has long since made the colour of the countenance an index of the disposition; and whilst the blooming lively front has been considered as the attendant of a generous and open temper, the pale lived complexion has been received as the mark of one that is cold, selfish and contracted.

We find this connection, however, to exist in a much more sensible degree, between the surface and particular parts of the system than others. The sympathy, as it is termed, between the skin and stomach is the most important, and appears to have given rise to the application of the generic term to connections of this nature. On it depend many physiological and pathological phenomena. The increase of the digestive powers by the external application of cold, the flushing of the face after a meal, the cure of sickness by a blister, of a diarrhœa by a flannel shirt, and many others, are to be referred to this important connection. This consent between the skin and the stomach, has been exemplified by two facts, so very decisive, it would be unpardonable not to mention them par-

ticularly. The first is the well-known experiment of Doctor Hartley on a dog, to which he gave the *nux vomica*, and then beat him severely. The action excited on the surface, prevented the operation of the drug on the alimentary canal, and no sickness ensued. The other is mentioned by the celebrated Darwin, and is still more to our purpose. ‘Two dysenteric patients,’ says he, ‘in the same ward of the Infirmary at Edinburgh, quarrelled, and whipped each other severely for a long time with horsewhips; both of them were much better after it.’ The success that attended Dr. Seaman’s remedy, in New-York, in a case wherein a quantity of opium had been taken, whilst it is a proof of this connection, is a further confirmation of the utility of external applications.

A second particular connection of importance, appears to exist between the skin and the mind, but, whether this is effected through the medium of the stomach, or is independent of it, may, perhaps, be doubtful. The remarkable connection between the stomach and the mind, so great as to influence the views, the desires, and the passions of man, and to have led, formerly, to the supposition that it was the seat of the soul, may account for the sympathy that exists between the skin and the mental part of our frame. That there is a connection, is proved by the effects of a fresh change of garments, mentioned before, and by the common observation of mankind. The effects of a cold wet day on our tempers as well as on our ability of mental exercise, must have been often experienced by all, and the influence of the passions on the skin, particularly of fear, is almost too obvious to need a remark. Let it suffice to say, that the melancholic temperament will still continue to be known by the hardness and dryness of the surface, and the choleric, on the contrary, by its softness and moisture.



Among the external applications in use at the present day, the principal are sinapisms, blisters, frictions, and various ointments, the manner of administering which, seems to have been left chiefly to the judgment or caprice of the practitioner. In a department of medicine, however, which, from the fore-

going observations, would no doubt be of the greatest utility if duly attended to, some care ought to be exerted in selecting those parts of the surface, which appear to possess a more intimate connection with the whole or with parts of the system. Are there any, that on this account deserve particular regard? Yes—the connection between the feet and the general system is so great, that they have been considered as the second greatest avenue of disease. To be convinced of this, we have only to reflect on the catalogue of morbid affections, induced by their imprudent exposure to cold or wet; and on the advantages resulting both in health and disease, from a careful preservation of their warmth, by the habitual use of flannel socks. Illustrative, and in proof of this connection is the practice of the untutored Indian, who, when obliged to pass a night in the woods, is careful to place his feet as near as possible to the fire, neglectful of the position of the rest of his body; and by a provident attention to this apparently trifling circumstance the lives of two persons were preserved, whose progress in an open boat, across the Delaware Bay, was impeded by the ice, and who were obliged to pass the whole of a severe winter night in this situation, without any other covering than what a single great coat could afford them*. This intimate connection has not escaped the discernment of physicians, and the feet accordingly have been the subjects of external application, whenever any powerful impression has been intended to be made on the system. It is not, however, sufficient to view them, merely as the parts to which a cataplasm or blister may be occasionally applied.

* As a more extensive knowledge of this circumstance may be the means of saving the lives of others, who are so unfortunate as to be placed in a similar situation, I shall take the liberly of relating it as delivered from the chair of the practice of physic. The gentleman who was present, finding that their efforts to reach the shore would be unsuccessful, ordered his companion to seat himself in the bottom of the boat, to pull off his shoes and stockings, and open his bosom. Having done the same, and seated himself opposite to him, each placed his bare feet against the breast of the other. Thus interlocked, with the great coat thrown over them, they passed a very comfortable night. The ice the next morning was sufficiently strong to enable them to reach the shore with safety.

Their connection with the system, entitles them to a more diversified regard. This is evident from the happy effects of Dr. Cullen's prescription in a case of obstinate costiveness; after every remedy had been tried without success, he ordered his patient, the duke of Argyle, to walk bare footed over a cold slab of marble—The consequence was an effectual catharsis.

Next to the feet, the wrists and ankles appear to be the most eligible parts for external application, as possessing the most intimate connection with the system, a pre-eminence that depends, probably, on the superficial situation of the blood vessels. Is the spine, in this respect, entitled to any particular regard, as the country practice of curing intermittents, by anointing it with turpentine, would seem to indicate? Of the importance of the epigastric region, so near the capitol of the system, I shall say nothing; to enumerate its points of superiority would be to relate the principal features of the animal physiology.

Before I enter on the consideration of the following particular external applications, it is necessary to remark, that the experiments relative to them, were all performed on a subject in a state of perfect health, and that consequently there should be some allowance for the greater difficulty of making an impression on the healthy, than on the morbid system. That this is the case, was evident from the greater length of time requisite for the sinapisms of mustard, garlic, horse-radish, &c. to produce their effects. Fully aware, moreover, of the fallacy of experiment, particularly as it relates to the mere *frequency* of the pulse, I underwent, what may be termed, a preparatory test, and found that by sitting still, with nothing on me but a flannel morning gown, the state in which the greater part of my experiments were performed, the frequency of my pulse was diminished twelve beats in a minute. It must however, at the same time be observed, that this diminution in frequency was not attended by a diminution in any other respect. The fulness of the pulse appeared to increase

in proportion as its frequency was lessened. These considerations will be applied by the candid reader to the individual experiments.

SWEET OIL.

In Italy there has existed, for many years, the practice of anointing the body with olive oil in fevers—a practice that has been strenuously recommended by the physicians of that country. This remedy has been used, not only in inflammatory, but also in bilious, and what have been improperly termed putrid fevers. Professor Murray, however, from theory I fancy, and not from observation, objects to it, and adds, as the reason, ‘that it cannot fail to debilitate the stomach, to produce nausea and vomiting, and from the strong smell which heat soon gives it, to corrupt the bile and increase the fever.’

Suspicious that a practice so ancient, and so strongly recommended, could not be perfectly inert, and to see the effects of olive oil when applied to the surface of the body, I underwent the following experiment:

Having breakfasted at eight o’clock—at ten, my pulse beating seventy-six strokes in a minute, its natural standard, I undressed entirely and threw over myself a flannel morning-gown, in which situation my pulse descended to seventy-two. About a quarter after ten, my whole body was rubbed over with oil, in twenty minutes after which there was a very perceptible diminution in the force of the pulse, though its frequency was little affected, varying between sixty-eight and seventy-two. At twelve o’clock the application was repeated, when a still further diminution in the force of the pulse took place—in twenty minutes after it was weak, greatly contracted in its volume, and its frequency lessened to sixty-two. This diminution in force and frequency continued till two o’clock, when a third application was made, with a further diminution of frequency to fifty-eight. Every stroke of the pulse was now perfectly insulated and distinct. At four o’clock, six hours after the first application of the oil, the diminution in force still the same, the frequency at sixty, an impatience

resulting from the irksomeness of the situation, made me put an end to the experiment.

I know not whether the increase or decrease of the irritative motions has any effect on our sensations, but about half after two, whilst the pulse was at fifty-eight, I never in my life experienced a stronger sense of the lingering lapse of time—the space of a minute seemed to be immeasurably extended. To impatience I cannot ascribe it, for the satisfaction arising from the issue of the experiment, as far as it had been tried, made me anxious for its continuance.

From this experiment, the rationality of the Italian practice is evident, and Professor Murray's apprehensions, of debilitating the stomach and corrupting the bile, turn out to be perfectly visionary. A quarter of an hour after the oil was washed off, I dined, and felt no difference in the strength and ability of the digestive organs. Twice was this experiment performed with the same result. Had time permitted me, I should have made a third trial with it, in order to have watched its influence on the different secretions.

Can we, from the result of this experiment, account for the practice of anointing the body with oil, so common among the ancients, and at this day, among the inhabitants of warm climates? It is used in this manner by the people of Africa, and in some parts of Asia; and Captain Cook informs us, it is a particular custom among the inhabitants of the islands of the Pacific Ocean. Mr. Niebuhr, in his travels through Asia, mentions it in terms that are very pertinent: 'The Arabs,' says he, 'pretend that this unction strengthens the body, and wards off the heat of the sun, to which, by going almost naked, they are very much exposed.' The ancients considered the external use of oil as essential to the preservation of their health and vigour, as is shewn by an anecdote related by Pliny, wherein Pollio Romulus, who was above an hundred years old, in answer to the question of Augustus, how he had so effectually preserved the vigour of his body and the powers of his mind, replies—'By the internal use of mulsum and the external use of oil.' Does it promote longevity, as Lord Verulam supposed, by preventing the too great perspiration and

sweat, or by diminishing arterial action, does it prevent the too great exhaustion of the excitability?

The external use of oil in fevers, has not been confined to Italy. It has been used, at Grand Cairo and at Smyrna, in the plague; and, also, according to Piso, in the fevers of South-America. Would it be serviceable in every state of fever? Its sedative influence evidently appears from the foregoing experiment, and there can hence be no doubt of its utility in fevers of great morbid action—but, would it not be of equal, nay, of greater utility in those states of fever, wherein there is a high degree of cutaneous excitement, and wherein the application can come in contact with the very seat of morbid action? It is a remedy so innocent, and the prospect of service from it so favourable, that a few trials with it should not be neglected.

Oil has been used externally in other diseases, as well as fevers, strictly so called, particularly in dropsy; and its success, in a number of cases, has been announced to the world, by Dr. Oliver, in the forty-ninth volume of the Philosophical Transactions. The same success seems to have been experienced both in Germany and France, though, with respect to it, as with all other remedies, there are exceptions, and cases in which it appeared to be entirely inert. Friction was, in every instance, conjoined with it, and it is difficult to say, from the known efficacy of friction in dropsy, how much is to be justly ascribed to the oil itself. Since, however, from a more just pathology of this disease, dropsy has been considered, not only as a consequence of fever, but a febrile affection itself, may not the oil by its sedative effects on the sanguiferous system, equalize the excitability and restore to the torpid lymphatics their natural proportion? In every case the quantity of urine was increased, a fact not to be accounted for by its merely moderating the action of the cutaneous absorbents. The cases related by Dr. Oliver are very remarkable. One of them was the consequence of hard drinking, at the age of fifty-seven, and another occurred in a woman at the age of seventy-two. Both of them were relieved. These cases should certainly lead to the more general use of a remedy so

mild and safe as sweet oil is, which cannot injure, should it not do any good, and especially when so little dependence is to be placed on what are termed diuretic medicines. Tissot supposed the efficacy of the external application of oil, in dropsy, to depend on its preventing the absorption of the humidity of the air, but Professor Murray, on this point, very justly remarks, that merely anointing the abdomen only, would, in this case, be of no avail, and that the application would be necessary over the whole surface of the body. Whilst the doctrine of absorption still remains doubtful, let us not have recourse to it, unnecessarily, to explain the action of a remedy whose results are so evident and so well tested by experiment and observation.

A great deal has been said on the efficacy of oil applied externally in that dreadful state of the system which is the consequence of the bite of a viper, and there is no doubt, from the foregoing experiment, of its utility in the fever that is formed; but that it alone should be sufficient, without the aid of more efficacious remedies, may justly be liable to doubt. On a subject, in regard to which there are so many different opinions, and to which there is no doubt attached a great deal of fallacy, I can do nothing better than give a place to the following very just observations of Professor Murray. ‘Vere venenatos fuisse plures, dubitare non sinunt peritia virorum qui experimentis adstiterunt, et effectus mali qui morsum exceperunt: quis vero veneno infestos fuisse omnes qui sanationem admiserunt, nos convincet? et quis, ejusdem malignitatis omnes et singulos venenatos esse, serio affirmabit? Ut igitur oleum quod unius speciei virus enervat, contra alius venenum nihil valere probabile sit! Variat præterea in una eademque specie morsus effectus pro profunditate vulneris diversa, pro parte vel magis vel minus nervosa, pro iræ, qua serpens succenditur gradu, pro copia salivæ venenatæ, et per aduncos dentes effusæ, *quæ post morsum unum et alterum inflictum parcius et debilius est*, pro numero vulnorum illatorum, pro majore vel minore sanguinis ex plaga effluxu, pro diversitate insuper temperiei animalium, vel caloris aëris. In tanto experimentorum numero horum momentorum aliquid facile potest neglectum esse.’ After these observations, it may be

well to remark, that Messrs. Geoffroi and Hanault, when commissioned by the Academy of Sciences, in consequence of the respectable testimony by which this subject was supported, to examine into the efficacy of olive oil in the bite of vipers, concluded, after a variety of experiments, that nothing could be affirmed of its specific virtue. Some recovered without oil, and others, on whom the oil was applied, died. A letter of a Mr. Miller, of Carolina, has lately appeared in the Medical Repository, in confirmation of the utility of this remedy. It stands, I believe, an insulated case in this country, and the credit attached to it should be regulated by this consideration. In respect to a disease, however, so fatal, and the issue of which is so rapid as to baffle our greatest exertions, every praise is due to a citizen, whose humanity led him to give such early publicity to a remedy, the effects of which, were apparently so beneficial.

Another disease, in which the external application of sweet oil has been of service is, diabetes. In the explanation of its effects here, also, recourse has been had to the absorbents; but diabetes, as well as dropsy, is a febrile affection, and in the cure of it, those remedies have succeeded best, which have directly diminished the action of the blood-vessels. Its utility, in this disease, has been established by the common practice of cure in Scotland, and has also been sanctioned by the authority of the late writers who have treated on it.

From the experiment, we may deduce the propriety of the domestic practice in applying oil to burns, wounds, and to parts inflamed by the bite of venomous insects. This observation will extend also to the use of the various mucilaginous herbs, gums, &c. which as nearly allied to oil, prove serviceable in the very same cases.

How does sweet-oil act? Does it prevent the action of the external air on the surface, which is to be ranked among those constant stimuli, the consciousness of which we lose through habit, or, 'as all parts of the skin,' says Abernethy, 'may be considered as the extremities of the body,' does it, by diminishing the action of the capillaries, lessen, also, that of the large vessels associated with them? Let the medical virtuoso decide.

ESSENTIAL OILS.

The external application of the essential oils has been much more common in the hands of the empyric, than in those of the real physician, by whom it would seem their efficacy has not been sufficiently estimated. However much the nostrums of the day deserve to be decried, as applicable to every state of the system and to every stage of a disease, yet there are hundreds of well attested cases of their utility, when properly administered. The British oil and the essence of mustard, amidst the numerous injuries that have, no doubt, resulted from a promiscuous application, it cannot be denied, have proved very often successful,.....a truth that should lead to the more frequent use, in similar cases, of those simple essential oils, which possess an equal, if not a superior virtue. That they are not inert, I conclude from the following experiments.

At eleven o'clock in the morning, my pulse at seventy-six, the neck, thorax, and abdomen, as low as the hips, were rubbed over with the rectified oil of amber. The effect on the pulse was as follows :

In 5 minutes, pulse beat 77				
10	-	-	-	78
15	-	-	-	78
20	-	-	-	78
25	-	-	-	79
30	-	-	-	80
40	-	-	-	82
50	-	-	-	82
60	-	-	-	82
80	-	-	-	82
100	-	-	-	83
130	-	-	-	82
160	-	-	-	82

In fifteen minutes after the application, there was an evident increase in the fulness and force of the pulse, and which continued to augment during the first hour. From this to the

end of the experiment, there was no perceptible variation. An increase in the heat of the body, and some fulness of head, accompanied the course of this experiment.

At half after nine o'clock.....my pulse at 74.....The neck, thorax, abdomen, and upper extremities were rubbed over with the spirits of turpentine.....within a few minutes after which, a sensation of pricking was felt over the whole surface to which it had been applied.....and an infinitude of maculæ, very similar to the first eruption of the measles, appeared, particularly on the back.

In 5 minutes, - Pulse beat 74

10 - - - - - 78

15 - - - - - 80

20 - - - - - 80

30 - - - - - 76

It was now re-applied, when in

5 - - - - - 74

10 - - - - - 76

20 - - - - - 78

30 - - - - - 78

40 - - - - - 76

50 - - - - - 76

60 - - - - - 74

80 - - - - - 70

100 - - - - - 66

The spirits of turpentine evaporated in about ten minutes after its application, which may account for its temporary effect on the frequency of the pulse, and for its not occasioning that increase in its fulness and force, which was produced by the oil of amber.

It is apparent, from these two experiments, that the essential oils, when externally applied, exert a stimulating effect on the system; and it has been the shortness of the time only, allotted for the preparation of an Inaugural Thesis, that has prevented my varying the experiments, so as to ascertain their proportionate influence. Their virtue in paralysis, conjoined with friction, is acknowledged.....What would be their effects

in epilepsy? Would not the oil of amber, if applied to the whole surface of the body, sometime before the expected fit, prevent, by the permanency of its stimulus, its recurrence? The spirits of turpentine, by exciting a cutaneous inflammation, would no doubt prove serviceable in many of those local pains, wherein the morbid action is not so great as to indicate depletion. It has often given relief in chronic rheumatism, and in those modifications of it termed Sciatica and Lumbago.

PERUVIAN BARK.

The external application of this celebrated article of the *Materia Medica*, if effectual, cannot but be considered as of the highest importance. Taken internally, it is by no means an agreeable remedy, and in some cases, there appears an almost insuperable antipathy to it. In children, moreover, it is frequently impossible to get a sufficient quantity taken, to produce any advantage. This, however, is not all. Of late years particularly in the most severe of our epidemics, the stomach has shewn a morbid irritability, that, by their premature evacuation, has prevented the due operation of medicines, taken internally. From this, therefore, and the additional consideration, that in the state of convalescence, the grand object of the physician is to exchange, as soon as possible, medical for culinary stimuli, the external application of this article if attended with success, will appear of infinite importance.

The Peruvian Bark has been applied to the surface in the form of cataplasm, fomentation, and the bark-jacket, the last of which is made by quilting two or three ounces of finely powdered bark in a silken or muslin handkerchief, which is to be worn round the waist. My experiments with it, in this shape, which were numerous and greatly varied as to the time of application, have convinced me, that it exerts a constant and durable stimulus on the system. It always raised my pulse three, four, or five beats in frequency, increased it in fulness and force, and augmented the heat of the body.

The issue of these experiments, has been justified by the tests of repeated experience. In the second volume of the London Medical Observations and Inquiries, there are related a number of cures effected by it in this manner. Dr. Barton informs me, that the venerable Rittenhouse prevented the recurrence of the intermittent fever, to which he was subject, by the constant use of the bark-jacket; and that in children, he has himself found it of essential service. So great in fact was its influence, that when, through oversight it was suffered to remain on till the complete formation of the paroxysm, it never failed to augment its violence. Cataplasms of bark, have also been used. In myself, when applied to the feet, they increased the fulness and force of the pulse, and prevented that diminution in frequency, which rest always produced. At the same time a disagreeable sensation of heat and dryness, was evident in the palms of the hands.

It is scarcely necessary to say any thing further on the utility of this article, as an external application. One thought however, suggests itself. In that stage of Typhus, or low state of fever, wherein the life of the patient depends on the hourly administration of the cordial draught, more necessary during the night than the day, and which is too often trusted to the presumed accuracy of an uninterested nurse, how often might a valuable life be preserved by the constant stimulus of the bark-jacket, exclusively of its good effects, as a continual application?

OPIUM.

The external use of opium, is supported by the venerable names of a Berghius and a Cullen—The former has ascribed to it considerable effects, and has even gone so far as to assert that it will vesicate. Dr. Cullen says, he has often found the external application of it relieve the pains and spasms of the stomach and intestines. To this might be opposed the authority of Wedelius, Alston, and Crump, whose experiments with this article, do not justify the assertion of these respec-

table characters. By the kindness of my intimate friend Mr. Jenkins, two opiate plasters were applied to the soles of the feet, and permitted to remain on for eighteen hours, without producing the least perceptible effect. Not satisfied with this, I applied, about four o'clock in the afternoon, an opiate plaster of four inches by five to the epigastric region, with an intention of suffering it to remain till the next day. I did not enter my bed in the evening, without some disagreeable apprehensions of the probable effects of the plaster before morning; my sleep however was not affected, and on removing the plaster about twelve o'clock the next day, it had not caused the slightest affection of the cuticle. What then are we to think of their frequent use; and shall we boldly declare, that both Berghius and Cullen laboured under a gross deception? In the annals of medicine, for 1798, there is an account of a letter from an Italian physician at Florence, on the external use of opium, in which he asserts that, 'an opiate ointment made by incorporating a drachm of finely powdered opium, with a pound of axunge, so that an ounce contained six grains, was effectual in a number of instances, and although the sleep produced, was not always proportioned, either in intensity or duration to the dose of opium, yet a state of calmness always succeeded. Certain of the efficacy of the opiate ointment, he also tried frictions of laudanum diluted with alcohol, and found them equally successful. I have not tried the efficacy of the opiate ointment; but my experiments with laudanum, have been many. In one instance, more than an ounce of laudanum was, at different intervals rubbed on the abdomen, without its producing the least change in the pulse; and in another the neck, thorax, abdomen, and upper extremities were rubbed with it, till an ounce and a half was expended. In this last experiment, the pulse was increased both in frequency and fulness, but this effect was very temporary, and the consequent depression was not such as indicated any previous considerably stimulant effect. Some fulness of head, and nausea also attended; but this was to be ascribed to the smell of the opium, which always induced these effects, in the person who was the subject of the experiment. Is laudanum then, externally applied, entirely inert? or are not the

experiments on the healthy subject conclusive and not to be admitted in contradiction to those made on the system, in the morbid state?—So great is the mass of evidence in favour of its utility, as an external application, added to the general use of it in this manner, over the whole medical world, that I am led to admit my own experiments, with the greatest diffidence.

TOBACCO.

This plant externally applied, promises to be of great utility as an emetic. Its efficacy has been particularly experienced in those cases, wherein poisonous substances have been taken into the stomach, the irritability of which, has suffered so much from the unnatural excess of stimulus, as not to be excited into vomiting by the usual emetics. In a case of this kind, after large doses of the antimoniated tartrate of potash, and the sulphate of zinc, had been given to no effect, the application of this plant to the region of the stomach, produced a sense of pain, hiccup, and finally vomiting. The operation of tobacco appears to be particularly directed to the stomach and intestines; and it is immaterial, the interval between the time of the application, and the vomiting excepted, at what part the application is made. Vomiting seems to be the invariable result. Plenck in his *Toxicologia* relates that a woman, in order to cure three of her children of the *tinea capitis*, anointed their heads with an ointment made of butter and the powder of tobacco; the consequences of which, were vertigo, vomiting, and syncope, that continued at intervals, for four and twenty hours. In another instance, the application of a decoction of the leaves, to a part on which there was an eruption, excited a vomiting of blood. The following experiment on myself, will show that it acts as a powerful emetic, though it is difficult to account for the interval that elapsed, between the application of the plant, and the period of vomiting.

At twelve o'clock some leaves of tobacco after being cut into small shreds, and steeped in some warm water, were applied to the epigastric region. In the course of half an hour it raised the pulse from 74 to 80 beats in a minute, though it varied as to fulness and force. A slight nausea now and then occurred, and continued to occur after the application was removed. It was suffered to remain on for two hours, at the end of which time the pulse was down at 70. The nausea was so slight as not to prevent my dining. I suspected indeed some defect in the tobacco, and had resolved to repeat the experiment the ensuing day. About four o'clock in the afternoon, however, the nausea returned in a much greater degree, and in the course of half an hour a violent vomiting ensued, continuing at intervals till ten o'clock at night. The quantity of bile discharged was immense. So great was the association formed between the smell of the tobacco and the retrograde action of the stomach, that I found it impossible to repeat the experiment with any degree of accuracy.

The utility of this plant as an external application in the cases above mentioned, is confirmed by the experience of Dr. Barton, who also thinks that its external use as an anthelmintic is too much neglected. A variety of other remedies applied to the surface have proved serviceable, and surely one, whose operation is particularly directed to the alimentary canal, is deserving of attention and trial. A fomentation of the leaves is said to be useful when applied to indurated tumours,

We now come to the consideration of the vesicating applications—A class of medicines which by their importance, so often the dernier resort of the physician, merit the greatest attention. Though of old date, they imply a greater advancement in the medical art than other remedies; for before man could be brought to suffer with indifference, the force done his feelings by their application, or reconcile the idea of curing one pain by exciting another, not only repeated experience of their efficacy, but some abstract reasoning would

seem to have been necessary. Whatever may have been their origin, or by whatever people they were first employed, their utility, sanctioned by the practice of ages, is not less confirmed by the test of daily experience. The common articles used for this purpose are the mustard, the horse-radish, the garlic, and the onion, to which are to be added from the animal kingdom, the American and the Spanish cantharides. To see their effects on the system, the following experiment was instituted.

At ten o'clock in the morning, my pulse beating 80 strokes in a minute, accompanied with some degree of head ach, two large sinapisms of strong mustard completely covering the feet, were applied to the lower extremities. They were suffered to remain on for four hours, more than one of which had elapsed before they occasioned any considerable pain. Very little variation in the frequency of the pulse occurred, if any, it was a diminution, but the increase of force, and particularly of hardness, was very considerable. About twelve o'clock the pain in my head and the disagreeable febrile heat I had experienced the preceding part of the morning, left me entirely. In the feet the increased glow of heat was very great, and towards the end of the experiment, resembled the sensation experienced by holding a part that had been scalded, near the fire. On removing the sinapisms and washing the feet, the pain ceased, but returned in about half an hour as violent as before, and I found it, during the remaining part of the evening, till twelve o'clock at night, disposed to remit and increase at an interval of from half an hour to an hour. The pain and redness were evident during the two succeeding days.

The influence of this experiment on my temper was very manifest. During its continuance I felt irritable, and peevish, and not much inclined to answer any questions that were asked me. I found it impossible to attend to the subject of the work I was perusing, and in counting the frequency of my pulse, was obliged to commence again and again, before I could perform it accurately. The exertion it now required being much greater than what was before necessary.

The effects of horse-radish and of garlic were the same, though in a less degree; how far this was affected by some

variation in the size of the applications, I will not venture to decide.

In addition to the above, I underwent the following.

Two blisters of the size of two inches by three, were applied to the inside of the legs, a little above the inner malleoli. They were suffered to remain on for twelve hours, at the end of which time they had produced an extensive separation of the cuticle, and a sore which required the delicate attention of the two succeeding weeks to heal. Their effects on the system were not by any means so powerful as those of the sinapisms; the pain arising from them was not so great, nor were the fulness, force or frequency of the pulse sensibly affected by them. We may hence draw the practical conclusion that blisters are chiefly suited for local affections, and that, when we wish to make a more powerful impression on the system, sinapisms should be resorted to. This conclusion I believe accords with the experience of every medical practitioner.

The importance of this class of medicine, is such, that they cannot be too particularly mentioned. The surprising effects of sinapisms in those states of the system wherein the most powerful internal remedies have failed, and that have drawn from the suffering patient an enquiry by what charm he had been relieved, will stamp them as a remedy of the first magnitude in the mind of every candid physician. Their utility has been particularly experienced in the convulsions of children from teething and from small-pox, in affections of the breast and stomach, and in what has been termed irregular gout. In the vertigo and strangury that are the consequences of a retrocession of the gout from the feet, they have afforded almost immediate relief—and who will not assent to their usefulness in those low states of fever wherein a powerful stimulus is indicated? What would not the application of two large sinapisms effect in preventing the recurrence of the intermittent fever? Blisters have been used for this purpose, and surely the more powerful influence of sinapisms would be proportionably more efficacious in destroying that association on which the return of those diseases appears to depend. In fact exclusively of local considerations, should not sina-

pisms more frequently supplant the use of blisters, as their effects are more powerful, their immediate consequences to the parts do not prevent a second application, and they are not followed by that disagreeable affection, which is so often the result of a blister.

Of the efficacy of the last, in diseases especially where there is a local affection, I shall say nothing—This subject has been fully treated of, by an antecedent graduate. I shall only observe in regard to the experiment, that the sore which was the consequence of their application, acted as a considerable irritation on the system.

CAMPHIRE.

If domestic practice can sanction the efficacy of any remedy, we must consider this as one of the most valuable articles of the whole *Materia Medica*, for external application. It is in fact, in this country, the family Panacea, and the bottle containing the camphorated rum, is resorted to with more faith and confidence in its utility, than the ancients used to attach to the influence of their domestic deities. As an external application, however, camphire has not been confined altogether to the private shelf, it is still held in considerable estimation, and frequently employed by the physician. Its power of occasioning the translation of gouty and rheumatic pains has been established, by a fact related by doctor Cullen, and has been further confirmed by the experience of doctor Barton. Its use therefore, should be attended with caution, especially where there is a disposition to irregular gout, or to effections of the more important viscera.

My experiments with this article, were made with the spirits of camphire, and with it in substance. The abdomen in one case, and the whole upper part of the body in another, were rubbed over with the former, and an agreeable sensation of heat and warmth was immediately perceptible over the whole surface to which it had been applied. The same sensation was excited in a greater degree, when it was applied in substance, in the form of cataplasm, to the feet.

In none of my experiments with it, however, though various, could I discover, that it exerted any effect on the pulse.

By this it appears that the principal utility of this article, as an external application, has been pointed out by domestic practice. What its effects may be in those states of the system wherein the excitability is accumulated, I must leave to a future more successful experimenter.

FRICTION.

The efficacy of this as an external application in many diseases is universally acknowledged. It is a remedy of an old date, and like sweet oil, has been used not only in the restoration, but also in the preservation of health. In some nations, particularly in China, it has become as necessary a part of their daily habits as the use of the bath or the razor; and an old gentleman is waited on by his *Iatralyptes* as regularly as he is with us by his barber. The effects of this process are said to be astonishing; that he who before its commencement was languid, dull, and inactive is rendered by it sprightly, animated, and nimble.

The ancients considered friction as highly important, and with them also it was the separate duty of a particular set of people. Hippocrates himself is said to have written a treatise on the subject.

The effects of friction on the human frame may be in some measure estimated by those of currying on the horse, which a farmer will declare is worth half his feeding; and that it is essentially necessary to the preservation of his health and vigour. Contrary to this, however it must be remarked that the celebrated Darwin will not suffer his horses to be curried. The reasons of a singularity so opposed to the common practice of the whole civilized world, are, I believe unknown.

Friction produces different effects according to the substances with which it is performed. In some cases the hand only is used, and proves often of great relief. Whether there is any difference between the hands of the two sexes, or how far

the soft delicate hand of the female may be more effectual, agreeably to Corporal Trim's experience, I am at present not able to determine. Next to the hands soft flannel, linen, and the flesh brush are to be successively used, and the intensity of the application gradually increased. From an inattention to this last circumstance it arises that in palsy this remedy so often fails in restoring the lost excitability.

In rheumatism friction has proved a very useful remedy. Captain Cook is said to have been relieved by it, of a violent attack of this disease, by the natives of Otaheite; and I am informed that a person now resides in Philadelphia, who has gained great credit in the cure of rheumatism, by the same remedy. He begins by gently stroking the inflamed part, and proceeds gradually to squeezing, pinching, and finally changing the inside for the back of his hand, makes some very severe impressions on it. His success it is said has been very great. In taking a view of the effects of this remedy, do not the virtues of the Metallic Points re-solve themselves into those of simple friction.

Dropsy, however, is the disease in which friction has discovered the greatest utility, and in which it particularly merits our attention. On this point Dr. Rush has mentioned three important cautions, the observance of which is absolutely necessary to obtain the good effects of this remedy—1. That the friction should always be upwards—2. Performed in a recumbent posture, and—3. In the morning only. A neglect of these rules will render its application of no effect, though continued for months.

In palsy and a variety of obstinate affections, such as the stiff joint and club foot, friction has been found of great service. In fact it is surprising what alterations a patient use of this remedy for a few months will effect.



The external application of vinegar, in which nitre has been dissolved, has been recommended in fever by Dr. Thornton, who is in the habit of ordering the bodies of his patients to be washed, and their arms to be plunged in a solution of it. Dr. Gregory often directs his patients, in what has been term-

ed the putrid fever, to be washed with a sponge dipped in simple vinegar and water; and says he has known it to reduce the pulse, from 110 to 90 strokes in a minute, whilst the delirium and other threatening symptoms have soon after disappeared.

The external application of cold water alone, has been found very serviceable in yellow fever, agreeably to the experience of Professor Rush—‘Cold water,’ says he, ‘was a most agreeable and powerful remedy in this disorder; I directed it to be applied by means of napkins, to the head; I also ordered the washing of the face and hands, and sometimes the feet with cold water; when applied in this way, it gradually abstracts the heat from the body, and thereby lessens the action of the system.’

How far does the application of cold water to the head, act as a preventive to disease?—On this subject, I hope I shall be pardoned for inserting the following. Standing one rainy afternoon at the door of my lodgings, I was struck by the conduct of an aged country friend, who, in walking down the street, took off his hat, and exposed his head to the rain. My curiosity was excited by an act, which I could not help instantly condemning, as the offspring of a foolish prejudice. On inquiry, however, I find it is a common practice in the country, particularly during the time of harvest, when covered with sweat, they are overtaken by a shower. An intelligent farmer assures me, that it has often secured him from colds and fevers, with which others who were present with him at the time, and who neglected this precaution, were afterwards seized.

I have thus brought to a close this imperfect sketch, of a few only of the many external applications now in use. I regret exceedingly, that an opportunity did not offer, of seeing their effects on the system in the morbid state. A variety of experiments, which I underwent, have not been mentioned, either from suspecting some fallacy in their issue, or from a

want of time to enable me to repeat them. If by this weak but honest attempt, I have removed one single impediment, from the path of a succeeding graduate, who may wish to enter on this fertile subject, I shall feel contented, and think the end of my dissertation answered.

With my best wishes for the prosperity of the university, and a respectful tender of my thanks to its worthy professors, I conclude this first essay of a truly medical Tyro.

Nec semper feriet quodcunque minabitur arcus. HOR.

A a

AN EXPERIMENTAL ENQUIRY
INTO THE PROPERTIES OF THE
POLYGALA SENECA;

SUBMITTED AS AN
INAUGURAL THESIS,

TO THE EXAMINATION OF THE

REVEREND JOHN ANDREWS, D. D. PROVOST
PRO TEMPORE,

THE TRUSTEES AND MEDICAL PROFESSORS OF THE UNIVERSITY
OF PENNSYLVANIA,

ON THE EIGHTH DAY OF JUNE, A. D. ONE THOUSAND EIGHT
HUNDRED AND THREE.

FOR THE DEGREE OF DOCTOR OF MEDICINE;

BY THOMAS MASSIE, OF VIRGINIA,

MEMBER OF THE PHILADELPHIA MEDICAL, AND OF THE
AMERICAN LINNEAN SOCIETIES.

'Et sentire quae velit, et quae sentiat dicere.

Hor.

INTRODUCTION.

AMERICA may be compared to a mine of treasures as yet imperfectly explored. To the ingenuity and talents of a few individuals the scientific world is much indebted; but in a field so spacious much remains to be done, and a great variety of objects present themselves to arrest the attention of the Botanist, Naturalist, and Physician. To the latter our country has furnished some of the most valuable articles of the *Materia Medica*, which are employed in opposing the ravages of disease. With the qualities of all of them we are by no means perfectly acquainted. The following essay is an attempt to explain more fully the properties of one of them, viz. of the *Polygala Senega*, which, from its abundance in the United States, and its efficacy in the treatment of *Cynanche Trachealis*, (a very frequent and often fatal disease) is entitled to our attention, even had it no other good qualities.

How far this attempt may succeed is submitted to the decision of the candid. The essay would have been much more complete, had not the season of the year prevented me from procuring any of the fresh plant, or of the varieties of the species, many of which possess properties very nearly allied to those of the *senega*. Should what I have written, however produce the effect of bringing this medicine into more extensive notice, and thus prove in any measure beneficial to mankind, the trouble and labour of preparing this essay will be considered as amply repaid.

The numerous imperfections with which it abounds, no doubt, furnishes ample room for the censure of the critic: But he should be silent when he reflects, it is the work of a youthful hand, actuated by necessity, not choice.

AN EXPERIMENTAL INQUIRY.

Among those vegetables, which Nature, in the luxuriance of her gifts, has bestowed on America alone, is to be classed the *Polygala Senega*, or Milkwort. This valuable plant grows abundantly in various parts of the United States, as in Jersey, Pennsylvania, Maryland, Virginia, and in the country bordering on the Ohio and Mississippi, where it obtains the name of Rattle-Snake Root, from its supposed efficacy in curing the bite of that animal.

Agreeable to the sexual system of Linnæus, it is arranged under the class *Diadelphia*, order *octandria*, genus *polygala*.

The character of the genus, as marked in the *systema vegetabilium*, is,

Cal. 5. Phyllus; foliolis duobus alæformibus coloratis; legumen obcordatum, biloculare.

Of the genus he enumerates twenty-six species, the sixteenth of which is the *senega*, and which is described thus:

—————floribus imberbibus spicatis; caule erecto herbaceo simplicissimo, foliis lato lanceolatis.

The *Polygala Senega* was first introduced to notice by Mr. Tennent, who learned its use from the Indians, and recommended by him as an excellent medicine for curing the bite of the Rattle-Snake, and for the treatment of pneumonia. After him Kiernander wrote a dissertation on it, which was published at Upsal in 1749 *. Since that period it has become more extensively known and used by physicians, of which

* See Medical Essays of Edinburgh, vol. 5. *Amœnitates Acad.* vol. 2.

further notice shall be taken in the subsequent pages. As yet the root only has been employed in practice.

In order to ascertain the effects of the powdered root on the arterial system, I instituted the following experiment :

EXPERIMENT I.

To my friend Mr. Lawrence, his pulse beating 65 strokes in a minute, I gave 20 grains of the powdered root of seneca in molasses, at 9 o'clock in the morning.—In five minutes his pulse beat 65.

Min.	5	10	15	20	30	40	50	60	70	80	90
Puls.	65	70	72	72	70	70	69	66	64	64	65

In ten minutes after the exhibition of the medicine his pulse increased in fulness ; and a sensation was felt in the œsophagus, which he compared to burning, with a considerable discharge of mucus from the trachea. In an hour and an half from the commencement of the experiment, all the effects of the medicine had subsided except the irritation in the throat, before mentioned, which still continued in a slight degree, and soon after quite left him.

EXPERIMENT II.

To my friend Mr. Liggett, with his pulse beating 65 in a minute, at 10 o'clock, P. M. I gave 20 grains of the powdered root suspended in water.

Min.	5	10	15	20	25	30	40	50	60	75	90	105
Puls.	65	76	80	82	84	90	90	82	80	80	86	86

Immediately after taking the medicine, the sensation in the œsophagus, mentioned in the preceding experiment, was very considerably felt, accompanied with a large discharge of mucus by hawking. In twenty minutes he complained of much heat in his stomach. In thirty minutes of nausea. In forty minutes the nausea was increased. In an hour and an half his skin became very hot. In one hour and three quarters a profuse perspiration commenced, and the disagreeable symptoms were quite removed.

EXPERIMENT III.

To my friend and fellow graduate Mr. Wilson, his pulse beating 68 in a minute, at 11 o'clock, A. M. I gave 20 grains of the powdered root.

Min.	5	10	15	20	30	40	60	75	90	105	120	150
Puls	72	72	76	78	80	80	78	78	78	72	72	68

Nothing peculiar occurred in this experiment, except the long continued influence of the medicine on the pulse, which was increased in force as well as frequency. The same irritation in the œsophagus, hitherto mentioned, was also very permanent.

Considering the power of the root of Polygala Senega in exciting the arterial system, is sufficiently proved by those experiments. I was next anxious to learn, in what principle the power principally resided. In order to accomplish this, it was first necessary to resolve the root into its constituent parts, which I supposed the following experiments would enable me to do accurately enough :

EXPERIMENT IV.

I submitted one ounce of the root in powder, with six ounces of pure water to distillation, and obtained in the receiver a clear transparent fluid, destitute of oil, and of pungency, but which had a peculiar taste and smell, compared by my friend Mr. Walker and myself to that of musty flour.

EXPERIMENT V.

I digested one ounce of the root in powder, with three ounces of very pure vitriolic ether, for eight hours in the sun. On separating and evaporating the ether, forty grains of a yellow resin remained in the vessel.

EXPERIMENT VI.

I put the remainder of the root, after it had undergone the operation of the ether, into an oil flask, with six ounces of distilled water, and exposed them to a boiling heat for half

an hour. The liquor, when filtered and evaporated, afforded me thirty grains of a dark vitrious friable matter, which I supposed to be gum.

EXPERIMENT VII.

To be certain that the matter obtained in the last experiment was gum, I dissolved twenty grains of it in water, then added gradually a drachm of sulphuric acid, diluted with an equal quantity of water, and no precipitate ensued, which, agreeable to Hærmstadt, proves the presence of pure gum.

EXPERIMENT VIII.

A solution of resin, and a solution of gum, tested by the oxysulphate of iron, underwent no change of color; neither did the resin, gum, or decoction, when tested by the alcohol of galls, shew any presence of iron.

EXPERIMENT IX.

In order to ascertain how long the root would furnish water with active principles, when boiled on it, I put one ounce with seven of water, in an oil flask, and exposed it for half an hour to a boiling heat; I then strained the decoction and added fresh water, which I continued in the same heat for the same length of time. It was much less colored than the first water, (which was very dark,) and possessed much less pungency. The water of the third boiling was nearly colorless and insipid. Cold water takes up very little of the active properties of the root.

EXPERIMENT X.

I accidentally mixed a portion of the decoction of the root with soap and water, a dirty green color was produced; but evaporation furnished me nothing more than revived soap.

I regret that I cannot say any thing of the other parts of the plant, the season of the year preventing my obtaining any of the fresh vegetable.

To examine the effects of the resin on the pulse, I performed the following experiment:

EXPERIMENT XI.

To my friend Mr. Towless, his pulse beating 72 strokes in a minute, I gave ten grains of the resin dissolved in spirit, at 9 o'clock in the evening.

Min.	5	10	15	20	30	35	45	60	80
Puls.	80	88	80	72	70	65	65	65	65

Immediately after taking the resin, he felt a great deal of irritation and roughness in the œsophagus, with a good deal of nausea. In ten minutes his pulse was much increased in fulness, with some irregularity. In forty minutes some irritation in the œsophagus still remained, with slight nausea. In eighty minutes all the effects had subsided, except, the depression in the pulse.

EXPERIMENT XII.

An amiable lady obligingly took a spirituous solution of twenty grains of resin at 9 o'clock. P. M. Her pulse beating 76 in a minute.

Min.	5	10	15	20	25	30	35	40	50	60	75	90
Puls.	88	88	92	92	88	80	80	80	78	72	72	76

In ten minutes she complained of a very disagreeable sensation in the œsophagus, and of a considerable degree of nausea. In fifteen minutes her pulse was much increased in fulness. In forty minutes the nausea subsided; but the irritation in the œsophagus still remained. In an hour and an half her pulse returned to the natural standard, and every disagreeable sensation disappeared.

EXPERIMENT XIII.

In order to try the activity of the gum, I took ten grains dissolved in water, at 7 o'clock in the evening, my pulse beating 84 in a minute.

Min.	10	15	20	30	35	40	50	60	70	80
Puls.	84	76	73	70	68	73	76	76	76	76

In five minutes after taking the medicine I felt a sensation in the œsophagus, as if the membrane had been abraded. This continued upwards of an hour; and to the extremely nauseous taste of the medicine I attribute the sinking of my pulse.

EXPERIMENT XIV.

To Mr. Thomas Redmond, a healthy boy, fourteen years old, I gave twenty grains of the gum dissolved in water, with the effect of vomiting him in five minutes in a copious manner; soon after vomiting he became quite well, and resumed his amusements.

EXPERIMENT XV.

At half past 11 o'clock in the morning, after a light breakfast, I took twenty grains of the gum, dissolved in water, my pulse beating 74 in a minute.

Min.	5		10		15		20		30		65		65
Puls.	74		74		74		74		74		74		74

The only operation of this medicine, was to produce the irritation in the œsophagus, so often mentioned; and to operate slightly as a purgative with some nausea.

EXPERIMENT XVI.

To my friend and fellow graduate Mr. Pendergrast, I gave at 11 o'clock, A. M. thirty-two grains of the gum in pills, his pulse beating 72 in a minute.

Min.	5		10		20		30		40		50		60		70
Puls.	78		80		82		76		74		74		76		76

In ten minutes he complained of much nausea; in twenty minutes the nausea was so much increased as to be quite distressing, and the medicine was with considerable difficulty retained on the stomach. In thirty minutes the nausea was somewhat diminished; in forty minutes still more so; in sixty minutes it was quite gone, and the gentleman in every respect in as good health as before the experiment.

EXPERIMENT XVII.

To a dog I gave half a pint of a very strong decoction of the seneca, made by boiling, three ounces of the powdered root in a quart of water to a pint. It produced excessive efforts to vomit, with the rejection of a large quantity of frothy matter. His stomach was quite free from aliment, as nothing was thrown up but this frothy matter.

In about half an hour after taking the decoction, a small quantity of blood distilled from the annus. During the experiment he was affected with a convulsive swallowing, and shewed a great deal of uneasiness about the larynx.

From these experiments it appears that the resin is more stimulating than the gum. They both have a strong tendency to produce vomiting, which I conceive arises in a great measure from the irritation they produce in the pharynx. Tickling with a feather or any other mechanical friction, will produce vomiting, and much in a similar way, I conceive these preparations act, in consequence of a peculiar and specific action, they exert on the œsophagus. That the operation of this medicine in the stomach, has an emetic tendency, I am far from denying; but I believe it would not so frequently occasion vomiting, unless assisted by the irritation before mentioned.

The best menstruum for the root of seneca is common spirit, which always contains a large proportion of water. The spirit will dissolve the resin, whilst the water it contains takes up the gum, and thus the combined powers of both gum and resin will be obtained.

Perhaps a strong dose of the gum or resin of seneca might be substituted with excellent effects for other emetics, in those patients whose stomachs are inirritable from having taken opium, stramonium, or any other narcotic poison. The practice of tickling the throat with a feather, in these cases, is often attended with success, when other means fail. The same irritation would be produced in a much higher degree by the seneca.

OF THE MEDICAL PROPERTIES OF THE POLYGALA SENEGA.

We have already shewn it is a stimulant to the arterial system, an emetic, cathartic, and expectorant; it often proves powerfully diaphoretic, diuretic, and sometimes salivates.

Dr. Rush, in his lectures on Clinical Practice, recommends the seneca as a diaphoretic, and Dr. Barton, in his Collections, says, 'this medicine sometimes operates so powerfully as a sudorific, that I have been assured it has been known to remove portions of the mucous body, or rete mucosum, from the skin of blacks who have used it.' He also speaks of its diuretic properties; and the ingenious Professor, in his Lectures on Materia Medica, relates a case in which the seneca produced a copious salivation, with a fœtor resembling that of putrid flour. With a view to produce this effect, I gave a young man the powdered root in pills, ten grains night and morning. During the time he took it, it proved considerably diuretic, but soon after having begun the use of the medicine he became very much indisposed from another cause, and was obliged to discontinue it, much to my regret, as I had not another opportunity of prosecuting this experiment.

We may here remark, how often salivating medicines act as diuretics. And as a reverse sympathy obtains between the skin and kidneys, so a direct sympathy prevails between the skin and the salivary system. This sympathy is particularly obvious in the small pox. Dr. Cullen observes, 'that in the distinct kind, salivation does not so often occur; but in the confluent it is an uniform attendant,' which seems to be in proportion to the morbid action on the surface of the body. In scarlatina anginosa, the inflammation in the throat, is coincident with the efflorescence on the skin, and the use of blisters in this disease, which is forbidden by Dr. Withering, is perhaps injurious, by the application of stimulus immediately to the part affected, which increases the associated morbid action, already too violent. This coincidence of action, is very apparent in the operation of those medicines which salivate as mercury, tartar emetic, opium, camphor, conium maculatum, squills, polygala senega, digitalis, and the nitric acid, all of

which exert decided powers on the skin; digitalis perhaps less so than any of the others; but even that agreeable to Dr. Darwin, whilst it depresses the pulse, produces much heat on the surface of the body.

The power of mercury is increased by a combination with most of those medicines, and I doubt not but experience will prove, that all possess the same property. The seneca has been already used by our Indians for the cure of syphilis*. This disease when it will not yield to mercury alone, is subdued by a combination of it with opium or conium maculatum, which probably arises from their tendency to act on the same system. And the celebrated power of the antimonial powder of the Pennsylvania Hospital is perhaps owing to the combination of nitre and tartar emetic with the mercury, they having a tendency to produce the same kind of action.

The reader will pardon this digression since it furnishes me a clue to explain the operation of seneca in scarlatina anginosa.

OF THE USE OF THE SENECA IN SCARLATINA ANGINOSA.

Dr. Withering has recommended the use of the seneca in this disease: 'among other remedies, says the doctor, I gave the seneca root, and frequently with advantage. But I soon had occasion to remark, that it procured relief only when it occasioned a copious flow of urine †.' If my observations are just with respect to the salivating medicines, viz. that when they increase the action of the vessels in the mouth and throat, they also increase the action of the vessels on the surface of the body. It is evident, that when the seneca acts in this manner, it can be of no service. On the contrary, I rather suspect it would do harm. But when it proves a diuretic, since a reverse sympathy obtains between the skin, and throat and kidneys, morbid action is invited from those parts to the kidneys, depletion takes place there, and the equilibrium of the system is restored. This opinion is further con-

* See Barton's Collections, p. 34.

† See Withering's Account of the Scarlet Fever and Sore Throat, p. 84.

firmed by the ill consequences arising from the application of blisters, before mentioned. It is worthy of remark, that the Indians also use the seneca in the malignant sore throat *.

OF THE BITE OF THE RATTLE-SNAKE.

As a remedy for the bite of the Rattle-Snake the seneca has been strongly recommended by Mr. Tennent and Kiernander, to be given internally, and applied externally to the wound. But more accurate investigation into its use has shewn, that the seneca has no more claim to the title of antidote, than any other of the numerous class of medicines, that have been ranked under that head. Fortunate, however, would it have been for mankind, if all the medicines which have been recommended for virtues, that did not reside in them, had possessed as many valuable properties as the seneca does.

This subject has been so fully treated by Dr. Barton, that for complete satisfaction on the vegetable remedies, that have been employed in America for curing the bite of this dangerous animal, I refer the reader to his paper, published in the third volume of the Philosophical Transactions, and shall only make a few quotations, which are directly to my purpose.

After having touched on the characters of some of the most reputed specifics, he observes, ‘ I was not ignorant that in the seasons of supervening langour and torpidity, the rattle-snake, in particular, bites with seeming reluctance, and without any, or with but little ill consequences arising from the wound. I likewise well knew, that even in those seasons, when the sun powerfully exerts its influence, at which times these animals are best qualified to strike and to injure, individuals of the species must often be found, the cavities of whose venomous fangs are entirely, or nearly destitute of their active poison, from the introduction of which into the system, those alarming symptoms which characterise the successful bite of this animal arise.’

* See Barton's Collections, p. 34.

These observations enable us to explain the manner in which the seneca has obtained the name of a specific. Persons have been bitten by those animals when they were nearly harmless, and their recovery after the administration of the seneca, has been attributed to its salutary operation; when, if it had not been used, no ill consequences would have ensued.

This interesting paper has also shewn, that after the evacuation of the poison from the fangs of the rattle-snake, it requires some days to accumulate again, and that animals bitten in this interval suffer little injury. Moreover, that among the western settlers, where those accidents most frequently occur, and where the seneca first obtained this reputation, internal medicines are by no means the only remedies used, but that recourse is always had to external applications, such as ligatures, scarifications, and blisters. And even supposing the poison had been introduced into the body, doubtless much more confidence is to be placed in the latter remedies, than in the internal administration of any.

IN DROPSY.

From the diuretic and diaphoretic properties of the seneca, it would seem a priori, a medicine extremely well adapted to many cases of dropsy, and experience has proved it such. Milman in his treatise on Dropsy, speaks in a very favourable manner of this medicine. Enumerating some of the most active diuretics, he adds, ‘ And here I cannot pass over in silence the seneca root, of which prepared according to the Edinburgh Pharmacopæia, I have given three ounces twice a day; but in this form it has generally excited vomiting, and discomposed the body very much; but when I have made use of only half an ounce of the root, in the same quantity of water, the medicine has proved a very good one; and although it sometimes produced vomiting, and often occasioned nausea, yet it generally purged nine or ten times in the day, and sometimes proved extremely diuretic.’ He afterwards relates four cases in which he used the seneca. The first a case of both

anasarca and ascites, in a young man, twenty years of age, was perfectly relieved. The second was mitigated by no remedy. On dissection the viscera were found in a diseased state, and polypi in the aorta, and vena cava.

The third case was of a hard drinker, who had been long afflicted with astma, and had then both ascites and anasarca. By the use of the seneca in conjunction with saline draughts, and the acetum scilliticum, he received some relief; but eventually perished.

The fourth I shall relate in the Doctor's own words: ' His thighs and legs were much distended, his hands and face were much swelled, the eye-lids were greatly enlarged, there was little or no water in the abdomen, the complaint had come on slowly, and encreased gradually; but he was cured by a decoction of the seneca root, in such a dose, as to procure four or five stools a day, and with the drink of cream of tartar.*,

My friend Mr. Hartshorn, apothecary to the Pennsylvania Hospital, politely furnished me with the following interesting case, in which the seneca appears to have been of infinite service.

Elisabeth Becher was admitted into the Pennsylvania Hospital on the 30th of October, 1802. She was affected with dropsy in its worst forms: hydrothorax, ascites, and anasarca. As I did not understand the language in which she spoke (German) I could not obtain an accurate history of her case; all I could learn was, that the dropsical effusion had succeeded a puerperal fever. She was bled $\frac{3}{4}$ x on the day of her admission, and began the use of the saturated tincture of digitalis, which was continued till the 4th of November, when, as it produced no change whatever, it was omitted.

The antimonial powders (composed of nitre, calomel, and tartar emetic) were now prescribed, with blisters to the wrists and ancles. These powders were exhibited every two hours for eight days, without affording any relief. Her mouth was very slightly affected. Her pulse, which had been so feeble, except on the day she came in, as to forbid the use of the lancet, was now scarcely perceptible. She was ordered de-

* Medical Tracts, vol. v. p. 92, 99, 105, 111.

coct. rad. senecæ, $\frac{z}{ss}$. every two hours, calomel continued. After taking the decoction three days she had a return of her catamenia; a profuse salivation came on, and the dropsy was very soon entirely removed. It was observed, that the salivation in this case was attended with a fœtor, very different from that which generally accompanies mercurial ptyalism.

Although mercury, in this case, may have been a very active agent, in producing salivation, yet from the peculiar fœtor which attended it, I am inclined to think, the seneca must have co-operated with it. And believing they act much in a similar manner, I can readily conceive, that their united power may have induced the ptyalism.

To these commendations of the seneca, the respectable authority of Dr. Percival* may be added; who says in hydrops pectoris, the seneca root, in liberal doses, sometimes answers every intention, and operates powerfully by the skin, the kidneys, and the bronchial glands, to the great relief of the patient.

From the very considerable stimulant power which the seneca possesses, I do not deem it proper in those cases of dropsy, where there is great activity of the arterial system, until the inflammatory action is subdued by the lancet, unless administered in such doses as to produce plentiful purging. But as soon as the pulse will admit it, I am persuaded it may be administered with the most sanguine hopes of success. Where the pulse is weak and languid, as in the case of Elizabeth Becher, it seems extremely well adapted.

IN TETANUS.

A case is mentioned by Dr. Barton, in his lectures, of Tetanus, brought on by the bite of the rattle-snake, which was perfectly relieved by very large doses of the decoction of polygala senega. The poison of the rattle-snake, I conceive acted here, as some of the vegetable poisons do, merely by its strong stimulant power. And that the seneca relieved the disease, by producing a new action, which from the great diffusibility of its stimulus, it seems well adapted to do.

* Percival's Essays, vol. ii.

My friend Mr. Towles, also informed me of a case, which he saw in Virginia of tetanus, cured by very large doses of the decoction. The cutaneous perspiration, he says, was so copious, as to run off the patient in streams. I exceedingly regret, that he had not the materials to furnish me with a correct and minute statement of the case.

IN PNEUMONIA.

In the first stage of pneumonia, in which a good deal of inflammatory action always prevails, I am convinced the seneca can be of no service, notwithstanding what has been said to the contrary. But in an advanced stage of the disease, when after plentiful evacuations, a difficulty of breathing, difficult expectoration, and pain, still continues, the seneca may be administered with much advantage. In this state of pneumonia Dr. Welford, a physician of Fredericksburgh in Virginia, informed me he used it with very favourable results; and that he learned its use from Mr. Tennent, the first introducer of the medicine into practice, who in these cases found from it the happiest effects.

In the pleurisy, as it is called, which prevails in many of the low and marshy countries of the United States, 'I do not doubt,' says Dr. Barton, speaking of the seneca, 'that it has been of real use.' This pleurisy or pneumonia, is a true intermittent, attended with local pain either in the side or in the head.

Bleeding in this disease is often necessary; but we must resort to stimulant medicines, after sufficient evacuations, for a radical cure; and the seneca may then be used with advantage.

Perhaps a combination of the powder of seneca root with Peruvian bark, may compose a medicine, superior to either singly, in the treatment of intermittents. by uniting the lasting and powerful stimulus of the seneca, to the tonic power of the Peruvian bark.

IN CYNANCHE TRACHEALIS.

Much praise is due to Dr. Archer, for the interesting discovery he has made, of the efficacy of the seneca in this disease. The formula he recommends, is the following:

R. Rad. Senek. in pulv. crass. ℥ss. coque in aq. fontan.
℥viii. ad ℥iv.

Of this a tea-spoonful is to be given every half hour, or hour, as the urgency of the symptoms may require; and at intervals a few drops to keep up the stimulus until it either acts as an emetic or cathartic.

The cynanche trachealis is a disease confined very much to the trachea, and removed considerably from the seat of the circulation, it often obstinately resists general remedies. The seneca, by the peculiar power it has of exciting the throat, as well as the general system, seems particularly adapted; first, to invite morbid action from the trachea to a neighbouring part, and then to diffuse it through the system. In the same way, I suppose, mercury acts, when it produces salivation, and thereby relieves the disease.

OF THE ACTION OF THE SENECA ON THE UTERUS.

In a letter from my friend Mr. James Archer, son of the celebrated Dr. Archer, from Harford county, Maryland, I received the following interesting communication: 'I can add one solitary fact of no trifling moment, respecting the effects of seneca on the female constitution; which is, that it will produce abortion or miscarriage. This was communicated to me last summer, by an illiterate man in this neighbourhood, well known for his strict adherence to truth.' Of one case in which it operated in this manner, in a very few hours too; he informed me, 'he had ocular demonstration.' He says, indeed, 'he has known and heard of many other like instances, in I believe all of which, it was taken by women who had indulged in illegitimate love, intentionally to destroy the fœtus in utero.'

The form of preparing it for this purpose, is that of decoction, made very strong, and given to the quantity of a very

large tea-spoonful, or more, but not less, at once. One dose, he says, has generally succeeded, unless they were very far advanced in pregnancy.

Taught by this relation, we should administer the seneca in a sparing manner to pregnant women, or omit it altogether. To those labouring under obstructed catamenia, it may perhaps be given with great advantage. And the case of Elizabeth Becher, before mentioned, whose menstrual discharge returned, after using it three days, tends much to confirm the opinion. That a medicine should act superficially on the uterus is not a solitary fact, in medical science. This quality has long since been attributed to madder, and the Adelia Ricinella, or Ram Goat of the West-Indies, possesses this power in an eminent degree.

But whilst sincerely reprobating the use of medicines for the unnatural purpose of producing abortion, it is also to be lamented, that so little indulgence is shewn to an excusable frailty, that the unfortunate victim of passion is compelled to have recourse to so horrid an expedient, to shield herself from the obliquy of an unrelenting world. The philosopher, who is acquainted with the force of human passions, and the weakness of the human heart, can pity and forgive the unfortunate girl, whom, too often the child of unsuspecting sensibility, is led astray by the seductive power of love. But, alas! there are few philosophers! The mass of mankind, governed by prejudice, have affixed irretrievable shame to a deviation from what they have termed virtue. And the child of nature who has wandered, to conceal her disgrace, has recourse to those remedies which too often bury herself and her infant, under the same ruin!

A DISSERTATION
ON THE
MUTUAL INFLUENCE
OF
HABITS AND DISEASE;

SUBMITTED AS AN
INAUGURAL THESIS,

TO THE EXAMINATION OF THE

REVEREND JOHN ANDREWS, D. D. PROVOST
PRO TEMPORE,

THE TRUSTEES AND MEDICAL FACULTY OF THE UNIVERSITY
OF PENNSYLVANIA,

ON THE FIFTH DAY OF JUNE, A. D. ONE THOUSAND EIGHT
HUNDRED AND FOUR.

FOR THE DEGREE OF DOCTOR OF MEDICINE

BY WILLIAM DARLINGTON, OF PENNSYLVANIA,

MEMBER OF THE AMERICAN LINNEAN, AND PHILADELPHIA
MEDICAL, SOCIETIES.

.....*Custom moulds to every clime the soft promethean clay.*

Armstrong.

INAUGURAL DISSERTATION.

‘ Man is a bundle of Habits.’

THE great and universal influence of custom, or repetition of action, upon the operations of the living body, has long been known and acknowledged by the observant part of mankind; but the application of this important principle to the prevention and cure of diseases has been too much neglected by physicians. The justly celebrated Doctor Darwin has, indeed thrown great light on the subject of associate motions, and the diseases of association; but much remains yet to be known respecting the power of disease in destroying the habits of the system, and also of the influence which the introduction of new habits, or a recurrence to old ones, where they have been violated, may have in *removing* diseases.

It is with a view of turning the attention of the accurate observers in medicine to this interesting subject, that I have selected it as the object of my inquiries in this inaugural essay, I am fully sensible of the difficulties attending a thorough investigation of it, and the necessity of much more experience than it was possible for me to possess, to do justice to it:— But a desire of seeing the subject prosecuted by extensive and ingenious practitioners, and of having the degree of its influence ascertained, induced me to adopt it.

The nature of the living body is such, that it can accommodate itself to a great variety of impressions which were at first disagreeable, and even prejudicial to it: A repetition of

them, agreeably to one of the laws of sensation, renders them less and less detrimental, until they finally not only cease to be disagreeable, but become necessary to the support of health and life.—When any habit has become thus established, the continuance of it may be considered as a proof of health, and a violation as one of the premonitory signs of disease. Many peculiarities, and unnatural operations often become habitual by subjecting ourselves to the influence of custom, or repetition of impression and action,—as is finely illustrated by the ingenious author of *Zoonomia*.

Doctor Cullen in his Lectures on the *Materia Medica*, observes, that ‘both temperament and idiosyncrasy may be variously affected by *custom*, insomuch that by this any temperament may be corrected, confirmed, obliterated, or even a new one induced.’—Custom, he defines to be ‘the frequent repetition of impressions on the system. Custom is often confounded with habit. *Habit* is only the *effect* of custom, as when frequent repetition of impressions *has given laws to the system*.’

By this distinction we may understand why, in many instances, customs appear to be easily prevented from taking effect,—and also why many attempts to introduce new *habits*, fail. *The repetition of impression is not continued long enough to give laws to the system*.—Hence we may perceive the propriety and necessity of long perseverance in difficult cases, where we wish to induce a new action that shall be permanent.

The explanation of the phenomena of habits is founded principally upon the theory of the celebrated John Hunter: viz.—That two different actions cannot exist at the same time in the same part, or system of the body;—and that the most powerful exciting cause will always produce an action of its own kind, to the extinction, or removal of the one previously existing. The correctness of this theory is so evident that it is now generally admitted, and taught in the schools of medicine.

The living body is endowed with a certain susceptibility, or aptitude for action, upon the application of stimuli, which is denominated *excitability*. This excitability may be defined a

condition of the system which owes its existence to a particular organization of matter. With this organization it is capable of being excited into action when stimuli are applied; but it is entirely *passive* as it respects the *kind of action* into which it may be excited. Every different stimulus has the power of producing an excitement peculiar to itself*. The ever-varying state of the system is owing to the great variety in kind and force, of the exciting powers applied.—However, when any particular impression is continued for a considerable length of time, or *repeated to a certain degree*, and is of sufficient *force* to produce its specific excitement, the previously existing action is destroyed, and a new one takes place which will often become *permanent*, even if the cause should be removed.

Some curious facts are related of the influence of custom in giving laws to the operations of the system,—as of the Idiot of Stafford, England, who, being accustomed to tell the hours of the church clock, as it struck, told them as exactly when it did not strike, by its being out of order. There is also a story told by Montaigne, of some oxen that were employed in a machine for drawing water, who, after making three hundred turns, which was the usual number, could not be stimulated by any whip nor goad to proceed farther.

* To avoid circumlocution I shall employ the word excitability as expressive of the *system in a certain condition*. When I say a stimulus acts upon the excitability and converts it into an excitement, I mean, that organized matter, possessed of this property, is thrown into action when an impression is made. This condition of matter is common to *all* bodies duly organized, and the various phenomena of life are owing to the variety in organization, and of stimulus applied. Hence I infer that in the *same system*, the variety of excitement is owing to the various nature of the exciting powers.

Although the system be passive in the abovementioned relation, yet it cannot remain dormant, or inert. If the more powerful of the usual stimuli be abstracted, the aptitude increases so as to be acted upon by the weaker; which cannot be all abstracted. This being the case, it behoves all who prize a healthy excitement to apply such powers, and such only (as far as practicable) as are calculated to produce this effect.

By the light of this principle, I apprehend, are the habitual operations of the body to be traced to their source. By it we may learn how the frequent and regular application of the durable stimulus of aliment is capable of preserving the habit of healthy excitement, in opposition to those causes which have a contrary tendency; and to which we are unavoidably more or less exposed. The stimulus of aliment, however, like many others, is of such a nature that it cannot establish a habit which will continue after the removal of the cause:—A constant and regular application is necessary to the permanence of the effect: hence any considerable interruption of the process leaves the excitability unemployed,—other less favorable agents take effect, and convert it into an excitement of injurious tendency.

The influence of any habit with respect to diet on the system, is very great, and should never be overlooked in the treatment of diseases; more especially in those of long standing, or of weak morbid action, such as chronic dyspepsia. L. Cornaro, who *dieted* himself very strictly, was possessed of good health whilst he observed his regulations, but when he deviated from his rules he found his health and temper much affected:—Many other facts evince a similar effect of custom in this respect; wherefore Professor Rush, among the *prophylactics* of apoplexy, and some other forms of disease, recommends to avoid becoming *habituated* to any *one kind* of diet, —for a change would then be dangerous; and few people can avoid changing it in the course of a life-time.

This theory is equally applicable to the operations of the mind,—or more properly speaking, of the *brain*; for the mind is as much an effect of the operation of external agents upon the brain, as vision is of the impression of the rays of light upon the retina. The knowledge of this unfolds to us the immense importance of my subject in relation to *morals*; and points out the necessity of an education that will give permanence to such *habits of thought* (and consequently of action) as are calculated to promote the happiness of man, and the welfare of society.

The susceptibility of acquiring habits of thought and action is much greater in youth than in adult age; because the excitability at that period of life has not been acted upon by any

stimulus long enough to establish its particular excitement, or to give a lasting and definite mode of action to the system; and therefore, the new impression has no old established habit to overcome. It is similar to the taking of a new country, or one inhabited only by a few defenceless savages, by a company of adventurers,—they meet with no obstacle in obtaining possession, nor in establishing what form of government they choose;—but if that country had been long occupied by a brave and well-disciplined people, the conquest would be extremely difficult, if not impossible. Hence it becomes an object of great interest, both in a national and social point of view, that the rising generation should be subjected, as far as possible, to such impressions as are adapted to produce an habitual excitement of patriotic and moral ideas.

The influence of habit upon the mind was long since known, and expressed in the following just and interesting admonition: ‘train up a child in the way that he should walk, and when he is old *he will not depart from it.*’

A healthy, or rational state of mind, depends upon correct perception, association, comparison, and induction, or inference from propositions:—But our perceptions may be correct, and yet from a too great frequency of heterogeneous impressions, the habitual trains of thought, or operations of the brain will be interrupted; and hence we may understand why Booksellers, reviewers, and all those whose occupations subject them to great variety and dissimilarity of mental exertion, are so apt to become meniacal upon the least predisposition*. From their distracted and unconnected state of mind, they might be said (if the expression were allowable) to be *habitually destitute of habit* in their operations. The probable means of restoring a regular train of thought will be hinted at hereafter.

There is also a derangement in mental operation which is induced by too exclusive an application of any particular impression which is calculated to excite wrong ideas. This is most frequently occasioned by great distress, or mistaken notions of religion.—In these cases, such a despotic habit of ac-

* Professor Rush’s M. S. Lectures.

tion is established in the brain, that most others are excluded ; and therefore the patients are forever dwelling upon one theme.

Absence of mind, as it is called, is to be explained upon the same principle. It is a continued and exclusive excitement of the brain which cannot be overcome by the usual impressions upon the senses ; hence some of the greatest personages, those who have prosecuted abstruse inquiries with the greatest success, have been most subject to it.

Having premised thus much concerning the nature of habits, and the principles by which I conceive they are to be explained, I shall next proceed to consider the influence which disease exerts over the more remarkable habitual operations of the system, and to notice the practical inferences which are to be made from an attention to it.

INFLUENCE OF DISEASE UPON HABITS.

The approach of disease may very generally be discerned by its causing a suspension, or sudden violation of the accustomed operations of the body ; and more especially in those who are subject to conspicuous or remarkable habits :—For example, in those who are affected with habitual discharges from the system ; as the hemorrhoids, issues, chronic diarrhea, and others :—Or in those who are accustomed to the use of tobacco, or other powerful stimuli. Under the head of habitual discharges may be considered the periodical evacuation of the catamenia ; the state of which is at all times an important index to the condition of the system, in females.

The suppression of those established evacuations, is generally considered as the *cause* of the diseases which follow ; but it is only a *symptom*, or *necessary consequence* of the operation of a more powerful incitant than that by which they were sustained, producing a different kind of action. Thus, when conception takes place, the menses are suppressed, because

there is a new excitement induced by the application of a more powerful stimulus; and the previous excitement, or habit, must necessarily cease*.

This principle likewise applies to the suspension of those habits which are acquired by the frequent application of stimulants;—the remote and exciting causes of disease produce a new action in the system, and thereby obliterate the former one,—doing away the necessity, or appetite for the accustomed stimulus: These effects so uniformly appear as harbingers of disease, that they ought always to give the alarm when observed. There is also an inaptitude to the usual actions: the excretions are affected, &c. &c.—A long list of premonitory signs of this nature, is given by authors, which indeed are generally so well understood, that all we have to do is to endeavor to make a proper use of them. By a due attention to them we may often be able to remove diseases whilst in their *forming stage*, which would otherwise baffle the skill of the ablest practitioners.

.....' For want of timely care,
Millions have died of medicable wounds.'

Armstrong,

When we have been exposed to the remote causes of disease, and perceive their effects upon the habitual operations of the body, we may consider them as the premonition of impending evil, and should take measures accordingly.

* As disease is only a different excitement from that which constitutes health, or that by which all the functions of the body are performed with ease and regularity, pregnancy may very properly be considered as a diseased state. (Vide Med. Repository, Hexade I. vol. vi. pages 31 and 150; where Doctor Vaughan has ingeniously and ably proved that pregnancy is a form of disease.) The violation of the *menstrual habit* is one of the first symptoms of the disease,—and health cannot be said to be completely restored until this habit re-appears, as it is essential to a perfect state of the female system. This does not take place upon delivery,—but the disease may, at this period, be said to have come to a *crisis*; and lactation seems to be the most eligible method of bringing the system gradually back to the healthy state of excitement; when it again assumes that important habit.

There are *two* stages, says Professor Rush, in which diseases may be prevented, *viz.* in the *predisposition* and in the *forming stage*. Where the system is predisposed to *great* morbid actions, such remedies as lessen the action should be used; as fasting, rest, gentle cathartics, or small bleedings; and particular care should be taken to avoid *irritating* or *exciting causes*. Where the predisposition is too *weak* morbid action, the use of gentle stimulants, as the pediluvium, and warm teas, will generally remove it.

Doctor Lockette, in his Inaugural Dissertation on the *Warm Bath*, speaks highly of it as a *prophylactic*; and it certainly has great efficacy in restoring an equable and healthy excitement, when used during the predisposition to, or even in the forming stage of many diseases. The value of warm diluting drinks, and gently stimulating vegetable infusions, was frequently experienced by my respected Preceptor during the prevalence of the yellow fever in Wilmington, Delaware, in the autumn of 1802. By the timely and judicious use of those simple remedies, he evidently avoided the fatal epidemic, by removing the predisposing debility, and preventing the formation of disease, after the fatigue of professional duties had favoured it.

There is an account of a comedian who lived to near the age of 100 years, and was never sick during that long period, in consequence of always going to bed as soon as he found himself under the action of the predisposing cause, or in the forming stage of disease. By inducing a gentle diaphoresis in this state, he restored that equilibrium, which a few days, or perhaps a few hours neglect, might have rendered a copious depletion, and a tedious confinement necessary to accomplish.

The approach of the dysentery is almost always announced to the patient by a violation of the habitual operations of the bowels; and, in this state, a gentle *cathartic* will generally prevent the formation of the disease. It was prevented in a company of soldiers during the revolutionary war, by causing them to drink *sea water* freely, which operated as a purge.*

* Rush's M. S. Lectures.

In the *forming stage* of catarrh, the free use of diluting drinks, and an abstinence from, or sparing use of cordial and stimulating diet, will almost universally remove the disease. It is said the celebrated David Garrick was in the practice of removing catarrhal affections, in this stage, by eating a salt herring; the object, and good effect of which was the thirst it produced, thereby inducing him to drink largely of diluting liquids, which gave the disease a centrifugal direction, and restored an equable excitement. This disease being one cause (and a very frequent cause) of consumption, should never be neglected.*

The accession of mania is generally to be foreseen by an obvious interruption of the habitual trains of thought. A diseased perception, or a derangement of any of the other operations of the mind, is very quickly manifested, either in conversation or in actions;—and when once observed, should be immediately attended to. In this early state, a proper application of physical and metaphysical remedies will often have the happy effect of checking the progress, and preventing the formation of this deplorable disease. It is much easier to repair the mind when but one pillar is deranged, than after the whole fabric is demolished.

As mental derangement, particularly that kind or grade called melancholy, or hypochondriasis, very frequently *originates from erroneous perception*, we need not, we cannot in such cases, expect a return of rationality until the perception be corrected: *ex. gr.* If a man admit the idea that he is composed of glass, it will be impossible to convince him by any *argument* that there is no danger of accident from blows or falls.—The idea of fragility will inevitably be associated with that of vitreous structure: therefore, to produce a radical cure or to prevent such cases from degenerating into confirmed habits of wrong action in the brain, we should aim at restoring a correct perception as soon as it is observed to be diseased.

*Rush's M. S. Lectures.

Erroneous perceptions very often require, and are relieved by the application of such remedies as alter the state of the system and organs of sense ;—but there are also wrong perceptions from the partial subjection of objects to the inspection of the senses. Sometimes it is necessary that several of the senses should *unite* in their report of objects to the sensorium, in order to produce a *correct* perception ; hence, it would be proper in all cases where we suspect this has not been the fact, to call in the aid of the other senses. Many diseases of the mind are rendered incurable, and many errors propagated in society by a neglect of this valuable, this essential rule.

Vice is a morbid excitement, or operation of the mind, or brain, which tends to injurious consequences in society. The will, or perhaps more properly, the capacity of volition, is acted upon by morbid motives, and a new excitement of pernicious tendency is the result. If those motives be repeated in their application to a certain degree, a vicious habit of thought and action becomes established, and the moral faculties are impaired. Here we see the necessity of a preponderance of such motives as excite to virtuous actions.

It has been said that education is not sufficient to establish habits of probity and rectitude which will endure through life, because there are many instances of parents of exemplary morals having children of dissolute, or exceptionable character :—But this assertion originated in an ignorance of the laws of the mental economy. It is true, such cases are often observable,—but the persons are constrained to moral acts by parental authority, and not suffered to exercise their reason on the subject. No motives are held out calculated to *volunteer* them into good actions. The will is rather suppressed ; and therefore, agreeably to the laws of excitement, is more readily excitable into wrong action, upon subsequent exposure to morbid motives.

‘ For Virtue is the Child of Liberty,
 ‘ And Happiness of Virtue ; nor can they
 ‘ Be free to keep the path, who are not free to stray.
 Beattie.

It is a mistaken notion, both in domestic and national governments, to make the dread of punishment the inducement to upright conduct;—There ought to be motives of such nature and force applied, as would ensure a virtuous volition; and this is just as practicable as it is to ensure a healthy excitement of body by a proper use of alimentary, and other suitable stimuli.

When a person of virtuous habits has been exposed to immoral causes until they begin to take effect, their influence may soon be perceived. His old habits will be violated by the formation of a new excitement; or a different direction being given to the will. In the predisposition to, or in this forming stage of vice, many persons by a proper management, might be arrested in their mischievous, and often fatal career. How many, in this stage of the disease, have been converted to permanent rectitude, by presenting motives to them of a more powerful and salutary nature!

The pathology of the mind presents a vast and important field for observation; in which it is the peculiar province of physicians to display the true principles by which the various phenomena are to be explained. Volumes would be requisite to detail and apply every circumstance illustrative of this beautiful theory of the mind, as taught by the ingenuous professor of the practice of physic in this university; but the limits of this essay render it necessary that I should dismiss the further consideration of the subject under this head, and proceed to consider the influence which a repetition of impressions may have in producing new habits of action, and thereby removing previous actions of morbid nature.

INFLUENCE OF HABITS UPON DISEASE.

Proceeding upon the principle of the unity of action in the particular systems of the body, and of removing one kind of action by exciting another, we may understand how a continuance, or repetition of those impressions which are sufficiently powerful to induce a new excitement, can eradicate the original morbid action; or, in other words, how habits of

wrong action may be removed by producing those which are more favorable to a return of healthy excitement.

In cases of wrong action induced by subjection to new impressions, the first object in the treatment is the removal of the cause; and if the excitability be not too completely converted into the peculiar excitement of the new stimulus, the effect will soon cease.—But if it have been so long applied that its particular action is confirmed in the system, that action will continue, even though the cause be removed. The system appears to acquire an independence from repetition, by which it can *of itself* continue those actions that could only be produced originally by the presence of the exciting powers. When this is the case, morbid excitement must be destroyed by such agents as will produce a new action.

If the morbid action be violent, it must be diminished before a new exciting power can be safely applied:—if it be more moderate, but of long continuance, and deeply rooted in the system, it will often be necessary to take an intermediate step, and eradicate it by inducing another disease; but one which will be afterwards much more easily removed. We have an instance of this in the treatment of old cases of syphilis,—we destroy it by bringing on a mercurial disease; and when we have completely removed the syphilitic habit by introducing this new one, such applications are used as are calculated to displace the latter, by producing the excitement of health. And lastly, when morbid action is feeble, and not of long standing, the diseased habit may often be destroyed by such exciting powers as are suitable to induce, *directly*, the action of health.

When any acute or recent disease is occasioned by an *excess* of those agents which are adapted to the support of health and life, it may be removed merely by restoring an equilibrium of excitement in the different systems of the body;—but where causes are applied which produce a pernicious excitement in every degree of force (provided they take effect at all) the case is widely different. No man ever cured syphilis by simply equalizing the excitement:—It must be destroyed by such remedies as have the power of producing a new and different action.

It is upon those principles, which were first systematized by the ingenious Hunter, that we propose to explain the action of habits upon disease ; or rather, the influence of one kind of action upon another ;—for an acquired habit is nothing but a *different action* rendered permanent by repetition.

To remove an established excitement of evil tendency, often requires the aid of extensive observation, and acute reasoning. There is a certain grade in the force of every particular excitement, above which it is not safe to attempt a change by the *immediate* application of different exciting powers ; hence it is generally necessary in the *beginning* of diseases to diminish the excitement of the system by depletion, before we can administer such remedies as are suited to restore healthy action, with advantage.

The neglect of a due degree of this has been the cause of much mischief in the practice of physic. It is a truly important part of the treatment ; and to the illustrious Professor Rush are we indebted for much light and information on this subject. By attending to the circumstance, we may be enabled to introduce new habits of action upon rational principles, instead of groping in empirical darkness.

In our regard of the effects of custom, in the management of diseases, we should never lose sight of those habits which are induced by the revolutions of the celestial bodies, by the return of the seasons, and our connections with mankind ; more especially the connections of friends and relations,* neither should we overlook those laws of the system which have been occasioned by any particular mode of living ; whether of dress, diet, drinks, &c.—nor of those established by any particular medicine, whether it have disagreeable or salutary effects. They all have their influence, which by attention may be converted into use, but by neglect are always liable to counteract our endeavours.

We see somewhat to this purpose in intermittent fever ; the disease appears to be sustained by the united influence of the remote cause, and the diurnal revolution of the earth. It has been supposed that medicines given just before the ex-

* Doctor Cullen.

pected paroxysm would have most effect in preventing it by exciting a different action; but Professor Barton informs us,* that he has often found the paroxysm aggravated by this practice; and that he has had most success in the treatment by applying the remedies immediately *after* the paroxysm, or for a considerable time previous to the expected period of the next one; unless there were danger of the succeeding one proving fatal; which is sometimes the case.

This would certainly seem the most rational practice; for in this case, there is but the influence of the remote cause to counteract, and therefore a new action would be much easier excited by the stimulus applied, than when it had to combat the above mentioned additional force. To prevent relapse, the application should be continued until the new action be established.

The unmanagable disease of epilepsy appears to be often rendered habitual in the system by the influence of the periodical revolutions of the globe. The scientific Professor of Materia Medica relates a case in his lectures, which was originally brought on by fright, but which became periodical; recurring daily about the same hour. He removed the disease by exciting an action, previous to the expected paroxysm, which was superior to the accustomed epileptic action.

The cure of intermittent diseases, or those which occur at stated times, is most commonly affected by some of the more powerful incitant, or tonic medicines;—but to show that there is nothing specific or exclusive in their operation upon the diseased action, we need only mention the fact, that they have all been occasionally removed by exciting a superior action of a different kind, without the exhibition of any thing internally.

Intermitting fever has frequently been cured by violent emotions of mind, or great bodily exertion; or, which is perhaps more effectual, by the united exertions of body and mind. Doctor Cullen prevented several paroxysms of epi-

* M. S. Lectures.

lepsy in a girl by threatening her with punishment if she had any more.

It is certainly our duty to remove diseases by those powers which will produce a healthy excitement, or one the most *favorable* to health:—Yet the above, and many other facts evidently show, that it is not any particular remedy *alone*, which will destroy an existing morbid action; but that *any* cause will do it which is sufficiently powerful to produce a new excitement.—*Therefore, the best medicine is that which will produce the best new action.*

The influence of custom in rendering motions or operations periodical, was well understood by Doctor Cullen:—‘An instance of this,’ observes that great man, ‘we have in sleep, which is commonly said to be owing to the nervous power being exhausted, the necessary consequence of which is sleep, *i. e.* a rest of the voluntary motions to favour the recruit of that power: But if this were the case, the return of sleep should be at different times, according as the causes which diminish the nervous influence operate more or less powerfully; whereas the case is quite otherwise; these returns of sleep being quite regular.’

This law of habit might be applied with great advantage, in conjunction with other means, in cases of morbid or preternatural wakefulness; by favoring a regular periodical return of sleep, this distressing affection would probably be often relieved, where it now resists the operation of the most valuable soporific medicines, promiscuously given.

Any habits of patients respecting sleep, should at the same time be always attended to; for instance, if they have been accustomed to sleep in the neighbourhood of a great noise, the continuance of that noise will be necessary to the production of sleep in them, however it might prevent it in others.

The same principle has been applied by Doctor Darwin and others, to the regulation of the alvine excretions. A regular stated time of evacuation, or even an *attempt* at evacuation, has been found effectual in obviating and removing a constipation of the bowels.

‘Custom, or repetition, gives strength to motion.’ ‘This,’ says Doctor Cullen, ‘is of considerable importance in the

practice of physic, though but too little regarded: for the recovery of weak people, in a great measure depends upon the use of exercise suited to their strength, or rather *within* it, frequently repeated and gradually increased.'

Doctor Rush has amply illustrated this principle by his own practice in the Pennsylvania Hospital, in the treatment of rheumatalgia and paralytic cases. Many of the patients affected with those disorders, have recovered their wonted strength, by beginning to lift small weights, and repeating the exercise every day, gradually increasing them.

As disease is a habit of wrong action, so I would reverse it, and say that all habits of injurious tendency are diseases; and of course require medical treatment. The management of them all requires a knowledge of the laws of the animal economy; otherwise the practice will be empirical, and the success fortuitous.

The habit, or disease of *drunkenness* is a truly pernicious one; and the treatment of it proportionally difficult and critical. The cure is becoming daily an object of more solicitude and importance, and to accomplish this *radically*, is the province of the observing physician. It has been treated successfully.— And were those principles, before laid down, properly enforced in practice, I have no doubt but we should frequently arrive at this grand desideratum.

When the use of an active or diffusible stimulus has become so habitual as to render it a necessary support of animal life, it is in vain to attempt the disuse of it, without at the same time substituting an equivalent. If this be neglected, the system languishes from defect of its accustomed support, and a train of symptoms ensue which are generally disagreeable in proportion to the power, or previous continuance, of the stimulus omitted. It is from this cause that we see so many fruitless efforts at reformation among the unhappy votaries of Bacchus. How often have the unfortunate subjects of the habit of intoxication, upon seeing its fatality, resolved, and pledged themselves to abjure it forever! But, alas! from an ignorance of the laws of organic life, they have

‘ Resolved, and re-resolved—yet died the same.’

The abstraction of so powerful a stimulus leaves the system

in a state of distressing langour and debility, in which it is not calculated to re-act under the pressure of business and domestic wants. In this situation they again have recourse to the insidious poison, in order to restore the pleasurable excitement they have lost. But it is an error pregnant with every evil. They restore an excitement which hurries the body to dissolution.—They follow a pleasure which allures but to destroy.

The disease of drunkenness is often kept up by customs, which might, with a little resolution, be easily broken. Many persons addicted to it, only get intoxicated at certain times in the day, or among certain companions,—others only at certain places of resort, or at certain festivals, &c.—and some only when they can procure their favorite potation *.

By observing those circumstances, we may be enabled to dissolve the charm by breaking a link in the chain of association, and substituting a new one which will finally, with proper management, establish a new habit.

Even when repetition has made the stimulus necessary to health, if the patient be convinced of its evil tendency, and resolved on reformation, we may render his determination effectual by inducing a new action with a different stimulus; and thus pave the way for a radical cure. If the habit have been of long standing, it may be proper first to excite a *new disease*, as was formerly mentioned. The patient may begin by only changing the *kind* of drink; afterwards he may substitute a powerful stimulus of a different nature; as opium, some of the metallic preparations†, or strong infusions of the vegetable bitters and incitants.

By thus gradually effecting a revolution in the habits of the system, he may descend by means of tonics, and moderate

* Rush's M. S. Lectures.

† How would a salivation do, where other circumstances do not forbid? There is no process which has a greater effect in revolutionizing the system.

Since writing the above, I have been informed by professor Rush that it was tried with success upon a gentleman in Virginia. Blisters, he likewise informs me, have been known to suspend this destructive habit.

stimulants, to a state of healthy excitement. Or if a continuance of some extraordinary stimulus be requisite, he can use such as will prove least detrimental.

My friend Doctor Gideon Jaques informed me of a case much to my present purpose, which came under his own care. He removed this destructive habit in an elderly man, by exciting a new action with other stimuli; which supported the system at the same time that they produced this effect. The principal substitutes on which he relied, were opium and seneka snake root, gradually diminishing the dose.

It is said that the celebrated Doctor Johnson intirely conquered a propensity for drinking ardent spirits, by the free use of strong *tea*. This, and other powerful vegetable infusions, might probably be often substituted with advantage for the leathen draught.

Drunkenness has frequently been cured by exciting disgust, shame, and other new actions. Religious impressions have often removed the habit likewise. These facts prove in this case, what was formerly proved by great exertion, or emotion, when considering periodical diseases. There is nothing peculiar or exclusive in the operation of the above mentioned remedies on the habit of intoxication:—*any* application sufficiently powerful, will support animal life in the place of the vinous stimulus; but those ought to be chosen which produce the best excitement: and when the cure is attempted by moral or religious impressions, they ought always to be aided by the proper physical remedies, otherwise the chance of a radical and permanent restoration will be much less.

All other pernicious habits, as the excessive use of opium, tobacco, &c. are to be treated upon the same plan. A substitute must be used to support animal life; and if the excitability be not too much destroyed by the long use of the stimulus, the system may be gradually brought back to the healthy state. But if the habit have been of such long standing as to induce a permanent exhaustion of excitability,—or, in other words, if the impression have been so powerful, and so durable, as to derange that exquisite organization which renders the body capable of re-action when the *usual stimuli* are applied, and if this derangement be lasting, it will be necessary

to continue the use of the substitute *. Therefore, all that we can expect in this case, is to remove one evil by submitting to what with a healthy person would be another; but which previous circumstances have here rendered necessary, and of course, comparatively salutary.

There is an account of some Turks, who, being at sea, had used all their opium, which from habit had become an essential support of life: In this dilemma they sustained themselves, at the expense of conscience, by drinking wine which they had on board.

As different stimuli produce different actions, and as they possess different powers of acting upon the excitability, it shows the propriety of being provided with a variety of stimulating agents, as recommended by professor Barton. They might probably be tried with advantage in cases of great exhaustion of excitability; for it has been found that medicines of apparently less active powers, will sometimes effect a cure, where more potent ones have failed.

This circumstance may lead us to understand why a stimulus which could remove a tenacious habit of action, may have its action removed in turn by a cause seemingly weaker than itself, one which could not destroy the first action; as is seen to be the case in the treatment of syphilis.

After continued use has rendered tobacco a necessary stimulus, it is not uncommon for those so accustomed to it, to vary the *form* of using it without any inconvenience. Those who chew can leave it off, provided they supply the defect by smoking or snuffing. But I have also known persons long habituated to chewing tobacco, to substitute *ginseng*, and other vegetable stimuli, with complete success; thereby obviating the disagreeable effects of the former.

* The susceptibility of action from the application of any stimulus being destroyed, in a great measure, by the excessive use of that particular agent, may we not plausibly infer, that the reason why small-pox and measles can only be communicated once to the system, is because the morbid stimulus which produces them destroys, permanently, the excitability; or that peculiar organization which is essential to the production of such an action?—The action of those contagions is so effectual, and of such specific nature, that the system is no longer excitable by them.

These facts likewise show the practicability of changing any particular habit, when attention is paid to the state of the system, and a new one at the same time introduced. This must be done; for one habit can only be destroyed by the introduction of another.

In the treatment of mania, as of all other forms of disease, it is necessary to attend to the state of the system. If the action of the sanguiferous system be violent in this disease, depletion is just as requisite as it is in a pleurisy; and in mania from physical, or mechanical causes, the cure is almost as easy. But where there is a morbid excitement of long standing in the brain, induced by causes which acted directly upon the senses, or where false associations have become established, the management of the system generally, can only be considered as a preparatory step towards a cure. Physical remedies, strictly speaking, can only act *indirectly* on mania from what are called *mental* causes: and hence, as those remedies are principally trusted to in *all* cases, we may understand why mania from mental causes has been found more difficult to cure. Besides, in this species of mania, the causes very often continue to act while the remedies are using:

The senses, like so many great rivers, expose the brain to the invasion of every inimical idea; and therefore while those sources of wrong action are unattended to, physical remedies may often be used in vain. It is frequently in our power to exclude some of the most unfriendly causes; and the channels which admit the enemy, are always open to convey the well-disposed.

It is the business of the practitioner to attend to this, and to prevent the operation of morbid causes, as far as possible, by removing those which can be removed, and making such impressions as are suited to introduce rational trains of thought.

Where the excitement is not violent or when it has been sufficiently reduced, we may gradually attempt to produce a new action by other causes. If the mind be exclusively occupied with one theme, as is most commonly the case in hy-

pochondriacal affections, the brain should be subjected to the various impressions which are afforded by agreeable society, and travelling. If it be void of connected operation, a cause should be applied which will produce a regular excitement; and continued, or repeated until this excitement be established.

We have many instances of the efficacy of this practice. A lady was maniacal at all times, except when she played at cards; her friends observing this, played with her by turns, until her brain had recovered its healthy action so completely that she did not relapse afterwards.

A judge was perfectly rational whilst on the bench; but as soon as he left it he became deranged.* In this case the pressure of business produced a rational excitement which overcame the previous wrong action; but it was not continued long enough to become permanent. Could he have been occupied in this way a sufficient length of time, no doubt he would have been perfectly cured, as in the case of the lady.

The good effects of business closely followed, or any continued impression of salutary nature, in restoring a regular train of thought, have often been experienced; and it is to be wished that more attention was paid to those remedies which act directly upon the brain. By observing that operation of the deranged mind which is most allied to rationality, and supporting it as long as possible by the proper impressions either conversation, objects, or both,—it is more than probable, we might often accelerate the recovery of maniacal patients;

.....‘For the attentive mind,
By this harmonious action on her powers,
Becomes herself harmonious.’

Akenside.

Vicious habits are to be treated upon the same principle. If the patient have not been so long subject to the impression of morbid motives as to impair the moral faculties, the disease will disappear upon simply withdrawing the cause. But if a habit of thought and action, which is of injurious tendency to society, be established, the mere abstraction of the cause will

* Rush’s M. S. Lectures.

not be sufficient. The diseased action will continue, independently of the original cause ;—and hence we often see persons in this condition commit vicious acts without any apparent motive ; or at least, from motives which would have no effect on others. In this case a change is only to be effected by the application of causes which are sufficiently powerful to excite a new action ; and on the nature of that cause depends the nature of the action produced.

If the vicious excitement be violent, it must be reduced before salutary impressions can take effect. This fact has received a negative proof from the experience of all ages, by the vain attempts which have been made to induce a healthy action, in such cases, by the violent remedy of ignominious punishments inflicted in public. Such treatment, instead of exciting the desired action, generally renders the case desperate. Like ardent spirit in an inflammatory fever, it hurries the system on to a state of gangrene and death.

But the propriety and utility of the above mentioned practice has received *positive* and ample proof, from the successful management of those dangerous cases, in the new jail in the city of Philadelphia. This valuable institution is in reality an hospital for diseased morals. If the action be too great to admit of the immediate application of salutary causes, it is reduced by putting the patient in the solitary cells ; and no treatment has ever been found to produce a state of mind so favorable for the introduction of a new and healthy excitement. This being premised, motives are then applied which are calculated to restore industry and integrity ; and they are continued until it is believed the habit is established.

This method of treatment has been found so effectual, that numbers have been discharged completely cured. They have returned to society with the character of honesty and sobriety which they have ever after retained.

It may be said that many have relapsed ; but no more happens here than is often seen to take place in mania, and intermittent fever. It weighs nothing against the principle. If the remedy had been applied sufficiently long, the cure would have been perfect.

That accurate delineator of the human mind, the immortal *Shakespeare*, was well apprized of the great influence of custom upon our actions ; as is evinced by the following lines :

‘ Assume a virtue if you have it not.
That monster, custom, who all sense doth eat,
Of habits devil, is angel still in this ;
That to the use of actions fair and good
He likewise gives a frock or livery,
That aptly is put on ; Refrain to night ;
And that shall lend a kind of easiness
To the next abstinence ; the next more easy ;
For use can almost change the stamp of nature
And either master the devil or throw him out
With wondrous potency.’.....

Religious impressions exert great influence upon the *immoral form of disease*. The morbid excitement is subdued, or diminished by the terror of future punishment ; and a virtuous action produced by the pleasing prospect which is held out, of the enjoyment of everlasting happiness. What has been said will point out the propriety, when a reformation is attempted, of first depicting the evils resulting from a destructive career, and then to pourtray the motives to upright conduct in all their alluring beauties.

RECURRENCE TO OLD HABITS.

Where the confirmed habits of the system are not attended with much inconvenience, it would be better to endure them than attempt a reformation ; and indeed under all circumstances they should be borne with, unless pains be taken to proceed upon the same principles as above directed : for an omission of the accustomed stimulus has been known to produce disagreeable, and even fatal consequences.

A patient of Doctor Hope was affected with vertigo, coma, &c. to an alarming degree, in consequence of leaving off the use of *snuff*, through complaisance to his wife. The doctor, upon inquiry was informed of the circumstance ; he advised the person to return to his former *habit*, which entirely relieved him. The same remark applies to habits acquired by

the repeated use of any other stimulus, or to any habitual discharge from the system ; and when guided by this principle, the treatment will be easy to understand in all cases.

The application of preternatural stimuli to the healthy body is at all times prejudicial, and often dangerous. They interrupt the usual operations ; and until the system accommodates itself to the force of the impression, its condition is more or less precarious. Resorting to the use of snuff has been known to cause *fatuity* ;—and by leaving it off, health was again restored. This should teach us to be cautious how we attempt innovations of this kind ; and also to make inquiry in every case where such a circumstance may be suspected,

The *re-appearance* of those habits which were attendant on the health of the patient, after they have been suspended by diseased action, is likewise a matter of great importance, and should never be neglected, nor overlooked by the practitioner who is ambitious to make an early and correct *prognosis*.

In all dangerous, or critical cases, the friends of the patient demand and expect the opinion of the physician respecting the event ; and much of his reputation depends upon the correctness with which it is given. By a nice observance of the healthy habits as they return, he may be enabled to predict a favorable issue, before the negligent attendant can perceive any alteration.

Sometimes, indeed, where the disease has been violent, and of considerable duration, some of the more remarkable habits have been permanently destroyed, as I have myself witnessed. But otherwise they are not ; and in *all* cases, upon a favorable crisis, it is the light of returning healthy operations which produces the dawn of convalescence.

I might here give a long list of the symptoms which indicate a termination of diseases in health ; but they are so amply detailed by authors, and so judiciously commented upon by Professor Rush in his lectures, that in me it would be a work of supererogation. Besides, any person who directs his attention that way cannot fail to observe them ; and observing them, he will soon learn to make the proper inference.

There is an additional consideration attending an observance of the first symptoms of convalescence, which is of still

greater importance; and that is, the more early use of restorative, or tonic remedies than is frequently thought advisable by the generality of practitioners. It is a point which requires great judgment, it is true, and mischief may often be done by commencing the stimulating plan too soon; but I believe a proper attention to the above mentioned circumstances would obviate this evil in a great measure.

That a neglect of them has produced evils on the other hand, I have no doubt. How many instances are related of patients breaking over the rules of regimen prescribed by their physicians, and indulging their appetites in what was strictly forbidden, not only with impunity, but with great advantage! Almost every experienced nurse can relate an instance of such transgression, whereby the patient has evidently accelerated his recovery.

In those cases, the habits of health have returned unattended to by the physician, and the patient, by obeying the early indications of convalescence, has rendered his recovery more speedy than it would have been by observing the formal regulations of system, under its present imperfections. By disregarding, or counteracting those indications, in the infancy of their return, we may often retard the cure, if not prevent it altogether.

The recovery of convalescent patients may be much promoted by attending to the recommendation of Professor Rush, of having every object removed from their view which can tend to prolong the disease by association. The sight of old plaisters, pill-boxes, remnants of juleps, &c. exert a very unfavorable influence upon patients of delicate constitution, where the recovery is tedious. They should be taken away as soon as they can be dispensed with; or if practicable, the patient should be removed to another room, where objects will be presented to him which are calculated to make him forget his disease.

The same observations may be extended to the treatment of mental and moral diseases. When the maniac discovers the least sign of returning reason, it should be encouraged and

assisted by every humane endeavour * :—And when the prodigal retraces with penitent steps the destructive path in which he has strayed, he should be received with open arms, and kindly directed in the way that leads to peace, prosperity and happiness.

I have mentioned very few of the instances in which the influence of custom is evinced ; but, if the explanation be correct, they can all be understood from what has been said ; and may be turned to our advantage in the practice of physic.

I shall now conclude these desultory observations, which I fear will suffer in the eye of the critic from the uncouth form in which they make their appearance :—But to close without a tribute of respect to the illustrious professors in this university, would be to suppress the warmest emotion of my heart.

To all of them I am much indebted for their personal kindness and attention. They have individually honored me with that regard for my improvement which at once evinces the patronizing hand of liberal science, and is calculated to ensure a grateful and permanent remembrance.

The enlightened professor of *materia medica*, in particular, has conferred obligations which it will ever be my duty and pride to acknowledge. To his polite and friendly instruction I owe that taste for the study of nature, from which I anticipate the most rational and lasting pleasures of my life. It is his happy attribute to fascinate his pupils with the sciences he so ably teaches.

It is high time for the haughty bigots of the old world to acknowledge and revere the scientific talents of America. No longer can it be said, to the reproach of our country, that all her productions are of an inferior order. As she has produced a Washington, a Franklin and a Jefferson, to wrest and pre-

* This is of importance to attend to. Maniacal patients should be released from chains and cells as soon as the case will admit of it. Mania which would otherwise have admitted of a cure, has, I believe, become *habitual* from a continued impression of those objects which were applied during the violence of derangement.

serve our rights and liberties from the grasp of trans-atlantic tyrants,—so has she given us a Rittenhouse, a Rush, and a Barton, to maintain our dignity and independence in the various branches of philosophy, medicine, and natural history.

AN INAUGURAL ESSAY,
BEING
AN ATTEMPT TO ASCERTAIN
THE CAUSE OF THE
EXTENSIVE INFLAMMATION,
WHICH ATTACKS
WOUNDED CAVITIES
AND THEIR CONTENTS.

SUBMITTED TO THE EXAMINATION OF THE

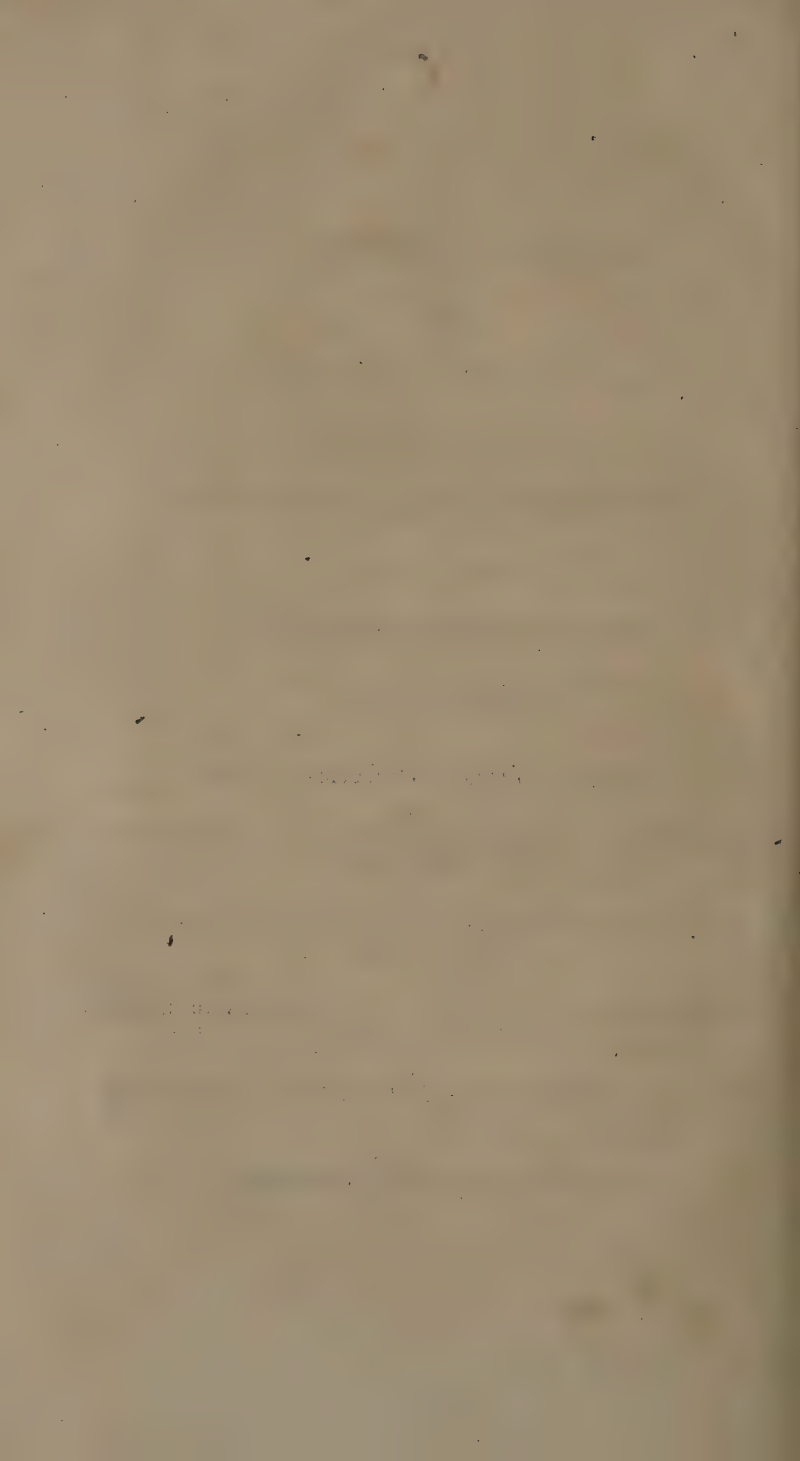
REVEREND JOHN ANDREWS, D. D. PROVOST
PRO TEMPORE,

THE TRUSTEES AND MEDICAL PROFESSORS OF THE UNIVERSITY
OF PENNSYLVANIA,

ON THE FIFTH DAY OF JUNE, A. D. ONE THOUSAND EIGHT
HUNDRED AND FOUR.

FOR THE DEGREE OF DOCTOR OF MEDICINE

BY JAMES COCKE, OF VIRGINIA.



INAUGURAL ESSAY,

In deciding on a subject for a dissertation, much difficulty presented. The great variety of diseases, which are interesting to physicians, have already attracted the attention, and exhausted the ingenuity, of other candidates for medical degrees. Independently of their productions, all diseases have been treated of, by medical writers of the greatest note, on whose descriptions and modes of practice, I could not have flattered myself, with the hope of making any improvement.

I have been induced to institute this enquiry, into the cause of the extensive inflammation which takes place on wounded cavities, not only from a consideration of the contrariety of opinions, which prevails among surgeons, on this subject, but by a hope, that I might bring into view, a source of this inflammation, which I think the true one, although it has heretofore escaped notice.

In the progress of this essay, I have been obliged to take a short review of the doctrines of the principal writers, who have touched on this subject. I hope I have succeeded in quoting their opinions fairly; and that I have in no instance, forgotten the respect, which is due to those, who have laboured to enlighten, and alleviate the miseries of mankind.

The greater danger of wounds penetrating into the cavities of the body, than of wounds of equal extent in other muscular or membranous parts, must have been noticed by the earliest practitioners of surgery. But, although the fact has been so long known, and admitted, by all writers who have mentioned the subject, no satisfactory explanation of it has yet been given.

An occurrence so frequent, so obvious, and so interesting, as the violent inflammation, which takes place on wounded cavities and on parts contained in them, could not fail to attract the attention of pathologists, and it has been attributed, by those of the highest credit, to different causes. None of their theories have, however, been so supported, as to gain general credence, and every surgeon embraces that, to which accident, prejudice or reflection has inclined him.

I shall not attempt to detail, all the hypotheses which have been advanced by authors, with a view of explaining it. Some of them, from the high authority by which they have been introduced, and from the ability by which they have been urged; demand particular attention. However opposite and contradictory the opinions of writers have been, as to the cause of the dangerous symptoms which supervene on a wounded cavity, no one has questioned the importance of ascertaining it, as it is evident that a discovery of it, would lead us directly to the most rational way of preventing them.

It appears to have been the opinion, generally received, from the most remote period of medical history, that atmospheric air has an injurious effect, when admitted into contact with parts, which are naturally defended from it. The opinion was simply announced, and as inflammation was always extensive, when any cavity was opened, and a communication with the atmosphere established; the air being the only foreign matter which could possibly insinuate itself, it seemed natural to conclude, that it was the offending cause; from this circumstance, I presume, the doctrine of the deleterious properties of air took its rise, and this theory the most universally acknowledged, appears for a long time to have been deemed incontestable, although intirely unsupported, except by a slender probability and the respectability of its advocates; nor do I find that any writer had ventured to oppose it, until within a few years.

This ancient and common opinion was adopted, extended and inculcated by Dr. Monro, who enters largely into the investigation, 'Of the causes of the dangerous inflammation

which generally follows the wound of a shut sac, and of the manner of preventing it.*

Influenced, by a consideration of the difference between simple and compound fractures and dislocations, by the results of a number of experiments which this author made, and by the issue of some cases, in which air was supposed to have had access to the cavities of joints, he concludes that the inflammation, which takes place on wounded cavities, as well as that following some of the principal operations in surgery, is chiefly owing to the admission of air, to which he appears to attribute highly stimulating powers.

That this very respectable writer was deceived, by taking only a superficial view of the subject, may, I think, be easily shewn. In the common accident of a fracture of the ribs, with a laceration of the lungs, the cavity of the chest is filled with air, from whence, it escapes into the cellular substance on the thorax, and is thence diffused through the cellular texture over the whole body; in these cases, unless the integuments are wounded and a communication made externally, no great inflammation is the consequence; although air in abundance is applied to the cavity of a shut sac, to a surface unaccustomed to its action, and on which it is said to produce highly stimulating effects: in what way has it here been rendered inert? and, why is it not uniformly followed by its supposed ordinary consequence?

Some experiments have been made by Mr. Astley Cooper, to decide, whether air is stimulating to internal surfaces. I do not know the particular manner in which they were performed, but from the known accuracy and ability of that gentleman, I have no doubt, of their having been conducted fairly, and of their having borne strongly and directly on the point, which he wished to ascertain: the result was, a conviction that air does not stimulate.

I am unable to determine, whether the pressure on the contents of the abdomen, by the diaphragm and abdominal

* Monro's Description of all the Bursæ Muscosæ, of the human body, page 39.

muscles, is so uniform, as to exclude the air entirely from that cavity when wounded, as has been asserted by Mr. John Bell, but as that cavity contains the intestines, filled frequently with flatus only, and composed of thin and yielding coats, it seems equally probable that air should get access to this cavity, as to that of the chest, filled with the lungs; which are well known, to be sometimes oppressed by air, entering through wounds of the breast. As a proof, that the cavity of the abdomen is replete with its contents, Mr. Bell observes, that hernia is frequently produced, by a blow upon the belly or by any sudden exertion.* That either of these, may be the cause of hernia, I am well aware, as a blow on the abdomen, may be given in such a way, as to make the bowels press violently on all sides; and if there is a weak part, it will yield, and some of the contents will be forced out of the cavity. On the same principle a bladder nearly filled with water, will burst, when forcibly stricken. That the abdominal muscles are capable of contracting so violently, as strongly to compress the bowels and to produce a rupture, cannot be disputed; but it is not more correct, to suppose the muscles of that part to be in their common state, thus contracted; than it would be to calculate that the gastrocnemii muscles are always acting with great force on the Tendo-Achillis, because they sometimes act so powerfully, as to rupture it.

If this pressure were so extreme and unremitting, the bowels would not be able to perform their functions; and the large and undefended vessels, especially the veins, would have their circulation arrested. One great source of alarm to Mr. Bell, is certainly ideal; he apprehends that the blood-vessels of the abdomen would burst, if not supported, by pressure on the surrounding bowels. That these vessels can bear unaided, the weight and momentum of the blood, is fully proven by their sustaining no damage, after child-birth or the discharge of large collections of water in abdominal dropsies. In neither of which cases can the flaccid parieties of the abdomen be supposed to have any influence on them.

* Bell's Discourses on wounds, part II. page 71.

The doctrine of air stimulating internal surfaces, during so long a period almost universally acknowledged, has been subjected to a close and severe examination by some late writers. As just and weighty objections could be adduced against it, it has been rejected, and attempts made by Mr. Hunter, Mr. Abernethy and Mr. John Bell each to substitute his own in the place of it.

The genius and celebrity of Mr. Hunter, as might have been expected, have engaged many of the most respectable surgeons of the present time, in favour of his opinion. The high respect which I entertain for the talents of these gentlemen, some of whom I have been accustomed to view, with that partial regard, which results from a sense of benefits conferred, of which I hold instruction to be the greatest ; and the veneration which I feel for the character of Mr. Hunter, induce me to hesitate when I think of attempting an examination of his theory, and of pointing out any mistake, into which, from the multiplicity of subjects engaging his attention, and the vast extent and importance of his discoveries and improvements in physiology and surgery, he may have fallen, in some points of inferior moment. Nor should I undertake it, if I were not compelled to write, and at the same time fully persuaded of the inaccuracy of his doctrine on the subject of this paper.

‘ In many cases of the emphysema, where the air is diffused over the whole body,’ says Mr. Hunter, ‘ we have no such effect, (as inflammation) *and this air not the purest*, excepting there is produced an exposure or imperfection of some internal surface for this air to make its escape by, and then this part inflames. Nay, as a stronger proof, and of the same kind with the former, that it is not the admission of air, which makes parts fall into inflammation, we find that the cells in the soft parts of birds, and many of the cells and canals of the bones of the same tribe of animals, which communicate with the lungs, and at all times have more or less air in them, never inflame ; but if these cells are exposed in an unnatural

way, by being wounded, &c. then the stimulus of imperfection is given, and the cells inflame, and unite if allowed; but if prevented, they then suppurate, granulate,' &c. *

These are the chief grounds on which Mr. Hunter has introduced the new, and to me, incomprehensible doctrine of the 'stimulus of imperfection;' which appears to have been assumed, without the semblance of proof, or any arguments shewing even the probability of the existence of any capacity in the animal economy to give it. It is, indeed, very certain that all wounded cavities are imperfect, and this being a necessary and unavoidable circumstance and uniformly attending; as it applies to every case, is, with perhaps something more of the appearance of truth, charged with the consequent inflammation, than air, which had before been suspected.

Mr. Hunter is not explicit as to the source of the *stimulus of imperfection*; nor am I certain; whether he intended to convey the idea, that it is given by an intelligent agent, but from the manner in which he speaks, he must have viewed it, as arising from some power capable of deciding when such a stimulus is necessary, directing its efforts to the benefit of the system and instituting such actions as are necessary to restore parts that are injured.

If this doctrine were in every other respect satisfactory, the introduction of this single term, which I am unable to familiarize to myself, by the substitution of any simple expressions, would be sufficient cause with me, for its total rejection; as well might we admit, the volition of nonentity in metaphysics. But, I am still more unwilling to admit Mr. Hunter's terms, from a recollection of the extensive injury, which science has heretofore sustained from theories, requiring the aid of imaginary intelligencies, to explain the operations of the human body, both in health and disease. They paralyzed investigation, and appeared to render superfluous the researches of the physiologist, and pathologist farther, than to trace causes to that point at which these agents might be supposed to commence their action.

Whether the cells in the soft part of birds, and the cells and canals of their bones are liable to become inflamed, from

the alternation of heat and cold, I am not certain. But, as other animals beside man, particularly the horse, are subject to catarrhal affections, by passing suddenly from a low to a high temperature of air, I think it entirely probable that these cells and canals, which are parts of the respiratory apparatus of birds, may also under similar circumstances be inflamed.

Other considerations however, have weight with me, as they shew the inadequacy of this theory. Emphysema, the very affection, on which Mr. Hunter has in a considerable degree, rested his conclusions, against the supposed effect of air in inducing inflammation, is intirely apposite to my purpose and contributes largely to disprove his doctrine. It shews that inflammation does not always occur to any considerable degree, from a cavity being rendered imperfect, and remaining so for some time ; which frequently happens when, from a fracture of the ribs, one lung and both sides of the pleura are torn, and a communication with the external air remains for several days ; and in such cases it cannot be denied that the cavity is imperfect ; both by a communication through the lung, trachea and mouth ; and also, by a laceration of the pleura costalis and intercostal muscles, giving passage to the air, into the cellular membrane on the trunk. Whence it is diffused over the whole body, which latter circumstance alone, seems sufficient to warrant the cavity being called imperfect.

When a wound is made into any of the cavities of the body, the abdomen for instance, every part of that cavity, as the integrity of the whole has been affected, should, according to the doctrine of Mr. Hunter, feel the stimulus of imperfection, and take on that action, which this stimulus is suited to excite. But generally, a portion of intestine, is protruded through the wound, and in a short time we perceive the peritoneum, which covers this piece of intestine, becoming more vascular, and incipient inflammation taking place on it ; while every other part of that cavity is entirely free from inflammation. Surely no good reason has been assigned, why a general cause should produce an effect so partial, nor does any explanation by the author of this doctrine occur to me, unless the stimulus of necessity is brought into action, which may be

said, to induce the inflammation, with the intention of rendering the cavity perfect, by producing an adhesion between the lips of the wound and the surface of the protruded bowel. But as the admission of this extraordinary agency, would be gratuitous ; I think it proper, to adopt a mode of accounting for these occurrences, which seems to be founded on a firmer basis.

The utility of the inflammation which takes place on the surfaces of wounded cavities, will not I believe be questioned nor will it be affected if shewn to arise from a law which pervades all animated nature, and not to be under the guidance of an intelligent principle or *anima medica*.

The inflammation, which generally follows the evacuation of matter from an abscess, and which, uniformly occurs in cavities that are laid open, could not escape the observation of Mr. Abernethy, who, ever close in his investigations and cautious in his conclusions, found it necessary, to abandon the theories which had been advanced by others, and to propose a more plausible one, which, attributes the inflammation to the *frequent renewal* and the *long continued* application of air, to a surface unaccustomed to it.

To this opinion of Mr. Abernethy, however, an objection presents itself. Although under ordinary circumstances his explanation may, and I believe will, be found to be literally true, yet I am certain that his intention was to inculcate the doctrine, that air, when frequently renewed and long continued, acts on internal surfaces as a stimulus ; which meaning may be fairly deduced from his words ; ‘ a constant renewal of air is permitted, and the application of a matter so unusual to these surfaces, I am inclined to believe does harm.’* Which doctrine, I think will not prove to be correct, as I am led to believe from a consideration of the mildness and pleasantness

* Abernethy’s Essay on Lumbar abscess, p. 55.

of air, when applied under certain circumstances, either to the internal or external surfaces for any length of time, and from an assurance, on high authority, that it is not stimulating to the animal fibre.

Of the various theories which have been proposed on this subject none has been more confidently advanced, than that of Mr. John Bell of Edinburgh. He assures us, that the 'inflammation running so quickly round all the surfaces of shut sacs, wherever they happen to be wounded, proceeds altogether from another cause, (than air) simple and plain to the last degree. For, in the wound of any shut cavity where the parts do not adhere, the inflammation spreads and runs its course, by a law of the animal economy, which we explain very ill, when we call adhesion the adhesive stage of inflammation, representing, as the first stage of a most dangerous disease, that adhesion which is a natural and healthy action, the most natural in all the system and the farthest from disease.' †

Simple and plain, as the cause of the rapid progress of inflammation in cavities, appeared to this ingenious surgeon, it is certainly to be regretted that he has not dispelled the darkness which to the view of most others, still rests upon this point.

The ideas of Mr. Bell, as to the cause of the inflammation, are more clear and accessible. 'It is plain,' he says, 'that inflammation, or the absence of it, arises not from the presence or absence of air, but from the length of the incision, there is no inflammation where the wound is small, though it is made on account of confined air; there is inflammation where the incisions are large, though they are made with the intention and also with the effect of letting loose the confined air *.

I am entitled by a variety of circumstances to deny, that the membranes lining cavities, are liable to become inflamed

† Bell's discourses on wounds, Part. II. p. 93.

* Bell's Discourses, part II. p. 90.

from slight mechanical injuries. In a number of experiments performed by Dr. Monro, he found that the inflammation was never in proportion to the size of the wounds made into the cavities of animals, but that it corresponded with the time and manner of the exposure of the bowels to the air.*

The capability of membranes lining the cavities of joints to bear violence without great inflammation, is shewn in all cases of dislocations, where the capsular ligament and consequently the lining membranes are lacerated. Similar hardiness is evinced by the pleura, which must frequently be considerably torn, by the rough ends of fractured ribs, and yet we see persons in whom it has happened, recovering without any of the symptoms of thoracic inflammation.

Inflammation from local irritation is generally proportioned to the extent of the injury, which is certainly not the case in wounds of cavities, the lining membranes of which, are sometimes extensively lacerated and no inflammation follows, while in other cases, the most alarming inflammation supervenes on a cavity, wounded only by a small puncture. Irritation from wounds, on which Mr. Bell lays so much stress in that discourse, in which he treats of inflammation of cavities, will not explain, why inflammation immediately takes place, on any portion of intestines, protruded through a wound of the abdomen. It surely will not be said, that the irritation or inflammation of the wound, has been communicated to the bowels by Mr. Bell's *sort of contagion*, and thus propagated over the whole of the peritoneum covering them.

If the inflammation of cavities were caused by the irritation of wounds, it would be expected to spread gradually from the wounds to the nearest portion of the lining membrane, and in a regular way to travel over the whole surface, which does not take place. But if any of the contents of a cavity are exposed, they immediately inflame over their whole surface, leaving in a sound state all that extent of lining membrane which intervenes, from the wound to the part protruded. In some cases, the inflammation of the intestines cannot be supposed to have the slightest dependence on,

* Description of the *Bursæ Mucosæ*, p. 45.

or the most remote connection with, the irritation of the wound. Of which, two experiments, made by Mr. Hunter with other views, furnish striking proofs. In one of these experiments, the surface of the testicle of a young ram, being exposed, became almost immediately more vascular, and in an hour or two discharged matter, different from that which is generally found on its surface. In the other, the cavity of the abdomen of a dog was laid open by an incision of several inches long; in five minutes, the increased action of the vessels of the cavity, had altered, and augmented the quantity of the lubricating matter; in fifteen minutes the surface was apparently more vascular, and the appearance of the secretion still more changed. 'The spleen' (and I suppose the other viscera) 'had its surface excessively red, from the increased number of small vessels carrying red blood. From these appearances' says Mr. Hunter, 'the fluid which lubricates the peritoneum seems to undergo changes, in consequence of exposure, and at last, when inflammation takes place, to have coagulating lymph substituted for it.*'

Before we can suppose this instantaneous effect to be produced by the irritation of the wound, it will be necessary to conceive, that its velocity is nearly equal to that of the electric fluid, and to attribute to it a power more extraordinary, that of inducing inflammation on one part of a surface, passing over that which from its continuity should next be affected, and seizing on another, and more distant part.

The irritation, from the puncture of a lancet, or even an incision, cannot be compared with that which is given to the tunica vaginalis testis, when a caustic has been applied to it, for the cure of hydrocele; by which application, that membrane, suffers all that it can do, from irritation, yet the inflammation is inconsiderable until the tunic has ruptured.†

If irritation from a wound or puncture were alone, sufficient to produce fatal inflammation; the violence done, by

* Hunter on the Blood, Vol. II. pages 144. and seq.

† Abernethy's Essay on Lumbar Abscess, page 56.

an incision made into the abdomen and uterus with the fragment of a butcher's knife, in the case related by Dr. Mosley and noticed in another part of this paper could not have been survived.

It is not my design to deny that irritation is the consequence of wounds, for I believe there is no wound, however slight, which does not produce some degree of irritation, but, I think we have ample grounds for deciding, that local irritation from a wound is incompetent to the production of inflammation so extensive as that which follows a wound into a cavity.

At a time, when cold was supposed to be actual matter, and thought to be a stimulant ; there could have been no difficulty in giving a plausible solution to the question, of the cause of the inflammation which attacks internal parts, when exposed to the open air ; as the air is generally below the temperature of the body. Cold had stimulating powers attributed to it, from pain, redness and inflammation of the skin having been observed to follow exposure to it, and by the system having been observed to be strengthened by the application of it, in a certain degree.

That the privation of an accustomed stimulus will produce pain is well known, and the violence of the pain will be in proportion to the extent and continuance of the diminution, within certain limits. This pain when caused by cold, arises from the actions of the exposed parts being lessened and performed irregularly, but pain from exposure to cold, is not very violent, until a higher temperature is applied. The manner in which it is then produced, will be noticed in another place.

That cold is not a stimulus is shewn by the weakness, torpor and death which are produced by its long continued application, by the effect which it has on the pulse and by its use in diseases of too great excitement.

The increase of strength, said to be produced by cold, as in the cold bath, does not appear during the application of cold,

and is entirely owing to the disposition of living matter, to become more sensible to its common stimuli, after they have been withheld for a short time. Which disposition does not shew itself in those cases only, in which heat has been applied, after its abstraction for some time ; but its effects are almost as striking in cases where persons have fasted for a long time. Some of the company of captain Bligh felt the symptoms of intoxication from eating oysters, and some berries of an innocent nature, after they had for several days eaten a very small portion of food. Dr. Percival mentions a young physician of Geneva, who, when a student at Montpelier, fasted three days, after which, the first nourishment he took was veal broth, and that had an intoxicating effect on him.

As all the above mentioned theories, appear in some part either defective or inapplicable. I shall make an attempt to account for the inflammation which occurs on wounded cavities and their contents, in a manner which has received the sanction of no surgical writer. But I hazard it, at present, with extreme diffidence, both on account of the hasty manner in which I am compelled to advance it, and the slight support which a very limited time allows me to derive from experiments, or from medical and surgical writers.

Before I proceed to suggest my ideas of the cause of inflammation supervening on wounded cavities, it must be observed that inflammation may be produced, 1st. By preternatural stimuli acting on a part possessing only its due proportion of excitability, in which consists its health ; instances of this are afforded by cantharides, some chemical preparations, heat of 212 degrees, &c. applied to the sound skin. 2ndly. It may be produced by ordinary stimuli acting on a part, the excitability of which has been accumulated by a temporary suspension of their action, which may be noticed in parts of the body which have been exposed to a great degree of cold ; it is also observable in catarrhs and some species of cynanche.

The effect of cold in rendering animal bodies more sensible to heat, or incapable of bearing so high a degree of it,

must soon have been learned from experience, by the inhabitants of cold countries ; and although they probably possessed no correct theory, as to its mode of action, they did not neglect to avail themselves of their knowledge in practice, as is shewn by the mode which they adopt, to recover persons who have suffered from exposure to violent cold.

That cold has the effect of making parts more sensible to heat subsequently applied, has been noticed and illustrated by Mr. Hunter, whose phraseology, when treating on this point, is new. He supposes the quantity of life to be lessened by cold, and says, stimulus must be proportioned to the quantity of life.

He says farther, ‘ cold, according to its degrees, produces two very different effects, one is the exciting of action without lessening the powers, the other is absolutely debilitating, while at the same time it excites action, if carried too far ; in the first, it becomes like suitable exercise to the vascular system, as bodily exercise is to the muscles, increasing strength ; but when carried or continued beyond this point, it lessens the powers and becomes a weakener, calling up the action of resistance after the powers are lessened.’.....
Again,

‘ Cold produces the action of contraction in the vessels, which is an action of weakness. A degree of cold suddenly applied, which hardly produces more than the sense of cold, excites action after the immediate effect is over, which is the action of dilatation, and which is the effect of the cold bath when it agrees ; and as cold produces weakness in proportion to its degree, its application should not be carried too far, for then it produces a much worse disease, irritability, or over action to the strength of the parts, and then indolence too often commences.’ *

Between the doctrine advanced in this extract from Mr. Hunter, and the opinion of Dr. Brown on the same subject, I perceive no particular difference, except in terms. They both allow that the effect of cold applied for a short time is the exciting of action, after it is withdrawn, or a higher temperature is given to it, which takes place on leaving the cold

* Hunter on the Blood, vol. ii. p. 74.

bath. When a part has been exposed for a long time to a greater degree of cold, the disposition to action is increased so greatly as to have inflammation or gangrene brought on by ordinary stimuli subsequently applied, examples of which are frequently seen in frosted limbs.

It was, I believe, first taught by Dr. Brown, that all sedative powers, weaken the tone of the fibre, which by accumulating irritability predispose to inordinate action on the application of a slight stimulus. Cold, I think, is eminently intitled to be considered a sedative power. That it produces debility is shewn by the effect which it has on the pulse and on the skin, also by the general reduction of strength which is observable in those who are exposed to it for any length of time. Of this debility, I presume, an accumulation of excitability or by whatever name it may be called, an increased aptitude to be acted on, to be the uniform consequence. An instance of this debility from cold and consequent sensibility to heat, is shewn in the common cold days of winter on our hands and faces.—When we first leave a room, the temperature of which is pleasant, and go into the cold air, the exposed parts immediately become pale and remain so for a short time, after which, from an accumulation of excitability, lessened tone, or weakness of the vessels of the parts, the action existing in the system will be sufficient to throw blood enough into these parts to render them quite florid; and if we return to the same temperature, which had before been grateful to our feelings, or to one a little higher, the action will be so much increased, as to give considerable pain, and even to go on to inflammation and mortification.

It has been conjectured that when any part of the body has been weakened by cold, the adjacent parts sympathise with it, and, as soon as the cold is removed, the action of the neighbouring parts spread to it, giving to it a greater degree of action than its weakened state can bear, of which, inflammation is the consequence, frequently terminating in gangrene. *

* Burns on Inflammation. Vol. I. p. 267.

It is not material to my present inquiry to ascertain, whether inflammation is induced by the sympathy just mentioned, or is the consequence of cold being applied beyond the powers of resistance of the parts, or is produced by stimuli acting on the accumulated excitability, which supervenes on debility. I incline, for various reasons, to the latter mode of explaining the phenomena, and one of the greatest weight, is, that it receives the support of all the arguments used by my preceptor Dr. Rush, in favour of his theory of fever; in either case, predisposing debility occurs, on this, an accumulation of excitability follows, on which any accidental additional stimuli, or even the ordinary stimuli will act with sufficient force to induce inflammation.

I think it strongly in favour of the truth of the explanation which I propose, of some cases of local inflammation, that it accords so fully with the theory of fever of Doctor Rush, which has thrown light on various diseases, and led to innumerable improvements in medical practice.

As the temperature of the internal parts of our bodies, is always considerably above that of the surrounding atmosphere, whenever a direct communication is made between the cavities of the body and the external air; it is obvious, that by a known law of heat, a reduction of the temperature of the cavities must be the consequence. These cavities, being uniformly accustomed to a heat of ninety-eight degrees, a reduction of that temperature by a number of degrees, which would have but little influence on the external surface, ever exposed to varying temperatures, may induce debility on the membranes lining the cavities; on which I suppose inflammation to follow on the principles which have been mentioned. I believe the redness, which is observable on the skins of infants a few hours after birth, to be a slight degree of inflammation, and think the sudden change of temperature, to which they have been subjected, is obviously the cause of it.

It is rendered more probable, that this is the true cause of the inflammation of cavities, when exposed to a temperature much lower than is natural to them, when the extent in which

the same principle may be observed to act, in the inferior orders of the animal creation and also in the vegetable kingdom, is taken into consideration.

All that vast number of animals which go into the state of torpor, strongly evince the extensive prevalence of a principle which is beginning to be generally understood and admitted, which is, *that animal matter becomes more sensible to its ordinary stimuli after they have been diminished or withheld for a short time.*

We learn on the authority of Fontana, that vipers which, during the winter had been kept at the temperature of 59 degrees, were destroyed in two minutes by exposing them suddenly to a temperature of 67 degrees; which is very far below that, which they easily bear and in which they are in full vigour during the summer. This fact is clearly in favour of the opinion, that cold has had the effect, on these animals, of rendering them infinitely more sensible to the stimulus of heat, than they were before they had been exposed to cold.

The Abbe Spalanzani observed, that newts conceal themselves in the earth and become torpid in the month of October, when the thermometer generally stands at 54 degrees; after remaining several months in this state, they re-appear in February, when the degree of heat is much less than at the time of their becoming torpid. No one, I believe, contends that there is any physical necessity, from the constitution of these animals, that they should go into the state of torpor; indeed it is known to be otherwise, as the same species of animal will be subject to become torpid in one climate and not in another*. They require a certain quantity of heat to keep them active, and this must be larger at the close of summer, than they can bear at the commencement of spring, furnishing one of innumerable instances, in which it is obvious that living matter becomes less sensible to stimuli which are frequently repeated. We here find the newts retiring on account of the defect of heat, when it is generally equal to 54 degrees; they

* Professor Barton's Lectures.

continue in their state of torpor, exposed to feeble stimuli, for several months; when, by some change which has taken place in them during the time that they have spent in their winter quarters, they are prepared to come forth in February when the temperature is frequently below that of freezing. That it is defect of heat which causes them to become torpid is shewn by their remaining active, when kept in warm places; and the change which takes place in the time of torpor, appears to consist in their acquiring a capacity, of having perfect life supported by a less quantity of heat. By which, I mean, the excitability of their systems has been accumulated and great action is produced in them by a slight stimulus.

This principle, so general and so frequently manifesting its influence, could not elude the accuracy and penetration of Mr. Hunter, to whom it appeared in a variety of familiar instances. He avails himself of it, to explain some phenomena which had not before been plausibly accounted for. He notices the disposition in eels to be so violently affected by a moderate heat applied to them when torpid, as to be destroyed by it in a few minutes. He reports the same effect having been produced on some other animals, as snakes and lizards. On the same principle, he explains the speedy death of birds caught during the winter, and brought suddenly into a warm room.

The immediate destruction of animals passing from a low to a moderate temperature, may, I think, be fairly attributed to the violent action which arises from the heat *suddenly* applied to their very excitable systems, because they become capable of bearing a degree of heat, when applied by the steady and unerring hand of nature, equal and often superior to that which kills them, when afforded by art, without a due regard to their particular states.

The effect of cold in disposing to increased action on the subsequent application of heat, is not confined to animals, but vegetables may be observed to have a similar effect produced in them, of which a variety of facts present themselves as interesting examples. It is particularly pleasing to notice the existence and influence of a law common to these two great orders of animated creation, forming another chain, by which

nature seems to have connected her works. This agrees with the many cogent arguments which have lately been advanced to support the opinion, that a gradation has been observed so regular and insensible, as to preclude the possibility of drawing a line of separation, and even to force us to admit, that there is no point at which animal life can be said to cease, and vegetable life to commence. Which doctrine is rendered almost certain, by the learned investigation, and the able and beautiful vindication which it has received from Professor Barton*.

In the vegetable statics of Dr. Hales, there are some very interesting experiments, by which the effect of cold in predisposing to action is clearly shewn. In the spring, when the temperature is still very little above the freezing point, the sap begins to rise in vines, and the force and rapidity of its motion are observed to be considerably increased by a cold night having preceded a warm day. If the sun rises clear, the sap continues to rise no longer than nine or ten o'clock, after which time, it was observed gradually to subside until evening; which, I think, shews, unequivocally that it is not the heat alone independently of circumstances which causes the sap to rise, as that is greatest after twelve o'clock. It must, therefore, depend on the irritability or excitability which is exhausted by the heat acting on it from sunrise until nine or ten o'clock; and the absence of the sun every night suffers the plant to accumulate a stock of excitability for the succeeding day. The irritability of the *hedysarum gyrans* is said to be exhausted by the noon day sun†. And the experiments of Fontana shew that the irritability of the *sensitive plant* is most abundant in the morning, less at mid-day, and almost imperceptible in the evening. It has also been remarked, that corn ripens in countries alternately cold and warm in a much shorter time than in such as are uniformly warm.

* Lectures on the affinities between animals and vegetables.

† I have here, as well as in several other places used the facts which have been collected in the third volume of Medical Extracts.

These, are only instances, in which the health and growth of vegetables are promoted by the alternation of heat and cold; but there are not wanting others in which, to continue the analogy, mortification and death are the consequences of a transition, too rapid, from the temperature of freezing even to that of a clear morning of spring. All delicate vegetables and the young leaves and fruits of trees, are liable to be blighted after a frosty night. That this arises from the stimulus of the heat of the next day, being disproportioned to the remaining quantity of life, or to the excitable state of the vegetable, and is not the necessary consequence of the frost, is proven, by the mode which has been successfully practised to prevent it; that is, by the simple precaution of moderating the heat by the evaporation which will arise from watering the plants before the rising of the sun.



Independently of the arguments which are afforded by analogy, drawn from the lower orders of animals and from vegetables; I am countenanced, in the opinion which I have advocated, by the facility with which it applies to a number of cases, which have not been explained on any other principle. One of these cases on which, I repose with great confidence, is that of the lungs, pleura and intercostal muscles being lacerated, by a fracture of the ribs. In this case the air escapes into the cavity of the thorax and into the cellular membrane; here no inflammation of consequence takes place, although air gets access to the cavity of a shut sac and should, according to Dr. Monro, produce inflammation: this air, is frequently *renewed*, that first taken in, finding its way into the cellular texture, it is also *long continued* in its application, and these according to the opinion of Mr. Abernethy are the two requisite circumstances, for the production of inflammation. I have in another part of this essay endeavoured to shew that imperfection of the cavity, supposed by Mr. Hunter to be the cause of inflammation, could not be denied to exist in this case; and certainly where a lacera-

tion of the soft parts has taken place to so great extent, there must be as much irritation as could be desired by Mr. John Bell.

Why inflammation does not occur here, is not satisfactorily explained by either of the writers, who have been mentioned. The reason is immediately obvious, when the theory which I have embraced is attended to, the air in passing through the lungs, the central point of animal heat, must acquire a temperature equal to that of the body ; and is thereby rendered unfit, to induce the debility on the cavities, which is necessary to predispose these parts, to take on inordinate action, on the application of so feeble a stimulus as that of their natural heat.*

But if, along with this fracture and laceration, a wound is made through the integuments of the thorax, then the surrounding air gets access to the cavity, without having had its temperature increased, and inflammation arises in the manner which I have already stated.

Nor is this effect of temperature by any means confined to a few cases of unfrequent occurrence ; but it may be seen almost daily in the practice of physicians ; all catarrhal affections are produced in this way ; and the range of this principle, includes all grades of action, from the transitory glow, which we feel in our nostrils, in passing from the cold air, to a warm room ; up to the fatal inflammation and gangrene, which take place on the application of heat after long exposure to intense cold.

* It might be contended here, that the air in passing through the lungs is deprived of its oxygen, and thereby rendered incapable of stimulating the cavity of the thorax. A considerable portion of the oxygen which is taken in, in healthy respiration, is well known to be expired in the same state, and this, when the lungs are wounded, must pass unaltered into the cavity of the chest ; but we have no grounds on which to found the opinion that oxygen, after getting into the chest, would unite with any thing there, and stimulate that cavity by the heat which would thus be produced : and I am acquainted with no fact, which favors the idea of oxygen possessing more irritating powers than any other gas.

There can be no doubt, as to the sensibility to the stimulus of heat being increased by a previous exposure to cold. The pain which is felt on holding the hand near a fire, after having been in the open air on a cold day of winter, or after handling ice, is familiar to every body. Which circumstance demands the admission of this principle for its explanation ; as the same degree of heat would have produced no unpleasant sensation in the hand, if it had not before been in a reduced temperature.

I think it probable, that I have the authority of Mr. Cline to support me in the doctrine which has been contended for in this essay ; but of this I am not certain. I recollect, that he attributes the inflammation of wounded cavities to a difference of temperature ; but whether he believes the doctrine of cold being a stimulus, or in what way he explains its action, I have not learned.

I am encouraged to adhere to the theory which has been proposed to account for inflammation arising in wounded cavities, by a consideration of the great advantage in practice which may be obtained from its being ascertained to be true. If the lowness of temperature should be proven to be the principal cause of inflammation on internal parts of the body when exposed to it, a number of operations will be performed without hesitation and with safety, which are now declined by surgeons, or undertaken with the most painful dread of the dangerous inflammation which they know must supervene. The manner in which I should guard against inflammation, consistently with my ideas of its cause, is plain, and immediately presents itself to view ; and I flatter myself that the proposition of Dr. Beddoes, to have rooms filled with air, modified in such a way as to suit the diseases of his different patients, will be considered a sort of precedent, and will shield me from ridicule, for suggesting the advantage that would probably result from performing some operations in rooms of a temperature equal to that of the human body.

The operations which would thus become less dangerous, are all those in which it is necessary to make an opening into

any of the large cavities of the body, for the discharge of substances contained in them, as in operations for empiema and those for the discharge of extravasated blood, and any viscid or solid matter from the abdomen; also, all the cases in which the cavities of joints are opened for the removal of fluids or cartilaginibus bodies; and the operations for hernia when it is necessary to lay open the sac. A mode of preventing inflammation on the exposed bowels in operations for hernia, which is pointed out by Professor Wistar, gives a tacit acknowledgement of the truth of the doctrine which has been advanced in this enquiry. He recommends bladders of *warm* water to be put on the intestines, which are unavoidably exposed in the operation: He has found this practice useful; its use could not be that of preventing air from touching the bowels, because no care could obviate it, besides it has been shewn, that air is not injurious to parts naturally defended from it. In these cases, I suppose, the heat of the water prevents the peritoneum from falling into that state of debility which would be followed by inflammation on the principles which have been advanced. To these, may perhaps be added, the operations for the discharge of large collections of pus.

The success of Mr. Abernethy's mode of discharging the matter of lumbar abscesses, may, I believe be attributed to the smallness of the punctures and their immediate closure, which prevent the temperature of their cavities from being reduced, and thereby obviate inflammation; the simple operation of paracentesis is successful for the same reason.

The cause of hectic fever consequent on opening lumbar abscesses and other collections of purulent matter, being entirely unknown; I may be allowed to venture a conjecture, that hectic fever is in no other respect different from the ordinary constitutional derangements arising from local inflammation except that which depends on the health of its subjects being injured by previous suppuration. It is, I believe on the same principle that the causes which produce inflammatory fevers in well-fed and healthy men, give rise to typhus fevers in sailors, soldiers, and poor persons who are

debilitated by having been badly fed and crowded together in confined situations, as on board ships, in jails, and in the miserable habitations of the indigent. In one case, debility is brought on by a tedious and distressing suppuration; in the other, by an habitual destitution of the necessary supply of food, &c. debility is also induced; which debility is of a chronic nature; and this, according to Professor Rush, expends the excitability of the system, leaving it in a state, in which stimuli generally act with too little force upon it to excite in it the commotions of (inflammatory) fever.*

Two other operations, which are so formidable as to intimidate the most intrepid operators, deserve the greatest attention from surgeons, and any proposition which affords the slenderest hope of their terrors being diminished, claims a candid consideration from those, whose standing in the profession entitles them to discountenance or to admit and encourage any innovation, proposed as an improvement.

One of the operations to which I allude, is the extirpation of schirrous and dropsical ovaries. I do not know that this operation has ever been performed, and as I had no authority to support me, I had scarcely dared to whisper my opinion of its feasibility, until I found it had been proposed a long time since by the venerable Dr. Shippen, Professor of Anatomy in this University. There is certainly nothing in the anatomy of the parts, which should deter surgeons from removing them when from disease they have become incapable of performing their office, and endanger life, or at least render it uncomfortable. We do not hesitate to take out cancerous eyes and schirrous testes when such sacrifices are necessary to preserve the lives of our patients. And, however strongly prejudice might oppose this practice in its commencement, I believe it would be submitted to, when the nature of the case, and the slight permanent inconvenience which would be the consequence of the loss of one ovarium, was fully comprehended. All surgeons sometimes witness the pain, anxiety, and despondency which are concomitants of these affections. The unhappy subjects of them despairing of a recovery, calculate

* Medical Enquiry and Observations, vol. IV. p. 127.

on spending their lives in a state of wretchedness and disease. Under these circumstances, no alternative however painful, which gives a prospect of health, would be rejected.

The urgency of the cases, and the rapid manner in which they hasten to a fatal termination ; have compelled surgeons to turn their attention to the distresses of parturient women ; who from malconformation, cannot be delivered in the natural way. For their relief, an operation has been occasionally performed, called the Cæsarian section which is perhaps the most dangerous one in surgery. A larger proportion of the persons who have been the subjects of it, have died, than of those who have borne any other accredited operation. The danger of this practice being considered, it is doubtless much to be desired that it could be dispensed with, this, however, seems impossible, and all that can be done, is to deprive it, of every portion of danger, which is not inseparable from it, and intimately connected with the parts which are the subjects of operation.

I know not, in what manner, the irritability of the uterus is to be estimated ; and am therefore, unable to decide, what degree of inflammation would arise from an incision made into it ; I presume, that a wound made in this part, would be equally liable to become inflamed from irritation, as one made in any other muscular part of the body. But as the influence of temperature, is so obvious, in causing increased action and inducing inflammation, mortification and death in living matter, it is at least probable, that the fatality which has almost uniformly followed this operation, may in some degree, depend on the exposure of the cavity of the abdomen, and its contents, during the operation. This operation therefore, which has heretofore been almost always only a prelude to death, has an indisputable claim to be performed in rooms of the temperature of 98 degrees. The danger cannot be augmented by it, but may possibly be diminished.

Among the very few instances of this operation being successfully performed, there is one reported by Dr. Moseley as occurring in the West-Indies ; a young negro woman, who was in labour, becoming impatient of the pain which she suffered, conceived the idea of relieving herself in a sum-

mary way, and with a common knife made an incision into the abdomen and uterus, through which the child was delivered. The intestines which had protruded through the wound, were replaced along with the placental portion of the funis, by some person at hand, and a stitch or two were put into the wound. The medical attendant, who soon after arrived, suspecting that some dirt or other matter had been thrust into the abdomen along with the intestines, took them out, examined and returned them, and at the same time extracted the placenta through the wound. This patient recovered, although almost every circumstance conspired to make the case peculiarly violent; the incision was made with a rough instrument, and the bowels handled and exposed much more than would have been necessary in a regular operation. As this case terminated favourably, under so untoward circumstances, it is fair to conclude that it ought to be attributed to something else than chance, and I can conceive no circumstance to which it may with so great probability be ascribed, as to the heat of the weather in that climate, increased probably by a still more heated cabin.

Mr. John Bell mentions a curious case of a soldier being wounded in the side by a halbert, afterwards walking a mile with his intestines hanging out, wrapt in the skirts of his shirt, and deposited in his hat: the roads were dusty, and the intestines dry as parchment and blackened with dust, when he obtained the assistance of a charitable old lady, who bathed his intestines with warm milk, and replaced them. It must be observed, that 'the weather, it being mid-summer, was intensely hot.' Here it appears that neither the dust nor other irritation was capable of exciting a fatal inflammation, which, I think, would certainly have been the consequence of exposure, so long continued, to a low temperature.

When I resolved to write on this subject, I flattered myself with the idea, that I should be able to prove the truth of my opinions, but was obliged to relinquish a series of experiments which I had conceived for that purpose, by the unfa-

yourableness of the season of the year, in which only, I had an opportunity of attending to the subject, and for the want of several conveniences which are indispensable to accuracy and success in experimenting. It is, however, pleasing to me, to reflect that the truth or fallacy of my explanation, is capable of being ascertained with absolute certainty; and I hope it will be deemed worthy of examination, by some able hand and more experienced experimentalist.

L I

AN INAUGURAL ESSAY
ON THE
LUPULUS COMMUNIS,
OF GÆRTNER;
OR THE
COMMON HOP.

SUBMITTED TO THE EXAMINATION OF THE

REVEREND JOHN ANDREWS, D. D. PROVOST
PRO TEMPORE:

THE TRUSTEES AND MEDICAL PROFESSORS OF THE UNIVERSITY
OF PENNSYLVANIA,

ON THE FIFTH DAY OF JUNE, A. D. ONE THOUSAND EIGHT
HUNDRED AND FIVE.

FOR THE DEGREE OF DOCTOR OF MEDICINE,

BY WAKEMAN BRYARLY, OF MARYLAND:

HONORARY MEMBER OF THE PHILADELPHIA MEDICAL
SOCIETY.

ROYAL CHURCH

(SOCIETY)

LETTERS TO

THE

ROYAL CHURCH

OF THE

ROYAL CHURCH

OF THE

ROYAL CHURCH

PREFACE.

I HAVE chosen for the subject of my Inaugural Dissertation, the *Humulus Lupulus* of Linnæus, the *Lupulus Com-
munis* of Gærtner, or the *Common Hop*.

I have thought it unnecessary to say any thing respecting its natural history, as it is diffused through every part of the United States, and is a native of both the old and new worlds. The cones are the only portion of the vegetable, which is used in medicine.

It would be much more congenial to my inclinations to glide through the process of graduation, in obscurity, than thus publicly to subject myself to the criticisms of the world : but as the laws of the University require a dissertation from every candidate for medical honors, I am compelled to submit this, to the inspection of the public : and hope that its being the production of a juvenile mind, impelled by necessity, and not led by choice, will apologise in some measure, for its imperfections.

Wm L. Black

~~William L. Black~~

CHEMICAL

AND

PHARMACEUTICAL TREATMENT.

EXPERIMENT I.

Four drachms of the lupulus were put into a glass vessel, into which twelve ounces of pure water were infused, and the precaution taken of keeping it well closed. After macerating twenty-four hours, it was put into a glass retort, to which a receiver was adapted, the retort was then exposed to a moderate heat in Blake's furnace.

In five hours, the contents of the retort were reduced to a dry cake, and a white transparent fluid had passed over into the receiver.

The distilled fluid had an empyreumatic smell, and possessed, in an eminent degree, the bitter property of the vegetable.

It was then tested by the tincture of litmus, by which it was converted to a red colour, but underwent no change of colour by the oxy-sulphate of iron; a variety of tests were then unsuccessfully tried, in order to ascertain what acid it was.

EXPERIMENT II.

One pint of water was poured upon four drachms of the lupulus, in an open vessel, and subjected to a gentle heat, till reduced to half the quantity; it was then passed through a filter; the fluid after percolation was yellow, very bitter, and emitted, in some degree, the odor of the plant.

EXPERIMENT III.

Six drachms of the lupulus were put into a glass bottle, to which twelve ounces of water were added; after macerating five days, with frequent agitation, the liquor was poured off and strained.

It was neither so bitter, nor so yellow as the decoction, but was endowed with more of the odor peculiar to the vegetable.

EXPERIMENT IV.

Four drachms of the lupulus were put into a glass bottle, into which ten ounces of highly rectified spirits of wine were infused. After the lapse of five days, in which time it was frequently agitated, it was decanted and filtrated.

The tincture was very green, and extremely bitter, and formed a white precipitate by the addition of water.

EXPERIMENT V.

Eight ounces of proof spirit were put into a bottle, to which two drachms of the lupulus were added. After macerating six days, the liquor was poured off and passed through a filter.

It was slightly yellow, and not so bitter as the alcoholic tincture, but more so than either the decoction or the infusion.

EXPERIMENT VI.

A decoction obtained by means of *experiment II.* was exposed to heat in an open vessel: it afforded me, after evaporation, a dark coloured bitter substance, which weighed one drachm and an half.

EXPERIMENT VII.

Four drachms of the lupulus were put into a bottle, to which a quantity of water, sufficient to cover them, was added. After macerating twenty-four hours, it was poured off, and another portion of water added; after repeating the affusion four times, it was strained, and exposed in an open

vessel, to heat: when evaporated to dryness, it afforded a residue of one drachm, of a dark, gummy, and somewhat bitter substance.

EXPERIMENT VIII.

Four drachms of the lupulus were put into a bottle, to which several portions of highly rectified spirits of wine were alternately added and poured off. It was then passed through a filter, and exposed in an open vessel, to the sun, for evaporation: it left one drachm and ten grains, of a dark, resinous substance, at the bottom of the vessel: it was much more bitter than either the extract or the gum.

EXPERIMENT IX.

To two drachms of the lupulus, in a bottle, six ounces of proof spirit were added. After macerating three days, it was poured off, and exposed, in an open vessel, to a gentle heat. There remained, after evaporation, thirty grains of a gummy, resinous substance. It was more bitter than either the gum or the extract, but not so much so, as the resin.

It appears, from the preceding experiments, that a moderate degree of heat is sufficient to volatilize the bitter principle in a large proportion; and that there exists in the lupulus, an acid, in a free state, which it would seem is *sui generis*. But I had not a sufficiency of time and opportunity to prosecute the investigation as far as I could have wished.

The odor of the hop is easily dissipated by heat, which appears from every experiment in which it was used.

The bitter resides much more in the resin, than in the gum.

EXPERIMENTS ON THE HUMAN SYSTEM.

EXPERIMENT I.

At ten o'clock in the morning, two hours after a light breakfast, I took twenty grains of the powder, suspended in water; my friend and fellow graduate, Mr. Mitchell, attending to the pulse, beating 80 strokes in a minute.

Minutes.	5	10	15	20	25	30	35	40	45	50	55	60	70	80
Pulse.	83	85	82	80	78	80	77	75	76	78	78	80	79	80

The powder was very bitter, and excited much nausea and burning in the fauces, which continued for a considerable length of time. In five minutes the pulse was fuller and stronger. In twenty minutes the pulse became languid, and the nausea increased very much. In 70, the nausea went off, and the pulse became natural. The flow of urine was very sensibly increased.

EXPERIMENT II.

At 11 o'clock, A. M. Mr. Mitchell took twenty-five grains of the powder, in a very small quantity of water; his pulse beating 62 in a minute.

Minutes.	5	10	15	20	25	30	35	40	45	50	55	60
Pulse.	63	60	58	60	60	59	51	56	52	53	58	60

In five minutes the pulse was increased in fulness. In fifteen there was much nausea with an inclination to puke. In 30 minutes the nausea and sickness became very considerable: in less than an hour it operated powerfully, as a cathartic.

EXPERIMENT III.

At five o'clock, P. M., two hours after my usual dinner, I took one ounce of the decoction, with my pulse beating 80 strokes in a minute, my ingenuous and worthy friend Mr. Merry attending to it.

Minutes.	5	10	15	20	25	30	35	40	45	50	55	60
Pulse.	80	82	81	80	80	80	80	80	80	80	80	80

The decoction was very disagreeable, and occasioned much nausea. At ten minutes, the pulse was somewhat fuller than natural, but suffered very little alteration.

EXPERIMENT IV.

At ten o'clock, A. M. I took forty drops of the alcoholic tincture, my pulse beating 74 in a minute; Mr. Merry attending to it.

Minutes.	5	10	15	20	25	30	35	40	45	50	55	60
Pulse.	76	80	83	81	81	79	77	74	75	74	74	74

In fifteen minutes, the pulse became fuller and stronger. At twenty there was a sensation of fulness and tension in the head, with evident narcotic effects. At twenty-five, the drowsiness increased and the pulse became very languid. At thirty-five, the drowsiness diminished, and at forty-five went off almost entirely. At sixty, the pulse resumed its natural fullness. The secretion by the kidneys was very sensibly augmented.

EXPERIMENT V.

At 11 o'clock, A. M. Mr. Merry took 80 drops of the alcoholic tincture, his pulse beating 58 strokes in a minute.

Minutes.	5	10	15	20	25	30	35	40	45	50	55
Pulse.	63	61	60	61	59	58	58	58	58	58	58

At fifty-five he took thirty drops more.

Minutes.	60	65	70	75	80
Pulse.	61	60	58	58	58

In five minutes the pulse was increased in fulness and strength: at fifteen the pulse became small and much drowsiness supervened: at twenty the system became languid, attended with profuse diaphoresis. At 30 the narcotic effects

were much less, and at 40 they went off almost entirely : at 60 the pulse was fuller than natural : at 70 it became depressed and languid. It operated on the urinary organs.

EXPERIMENT VI.

At five o'clock, P. M. I took one ounce of the aqueous infusion ; it was extremely disagreeable and excited much nausea, but had no sensible effect on the pulse.

EXPERIMENT VII.

At eight o'clock in the morning, Mr. Mitchell took six grains of the resin, dissolved in a very small portion of alcohol ; his pulse beating 70 in a minute.

Minutes.	5	10	15	20	25	30	35	40	45	50	55	60
Pulse.	73	76	76	78	74	72	72	70	71	70	70	70

In ten minutes the pulse was increased in fulness and strength ; at 20 he felt much elated ; at 40 he became languid ; at 70 the pulse resumed its natural force.

EXPERIMENT VIII.

At 9 o'clock in the morning, I took eight grains of the extract, dissolved in water ; my pulse beating 74 in a minute ; Mr. Merry attending to it.

Minutes.	5	10	15	20	25	30	35	40	45	50	55	60
Pulse.	76	76	75	74	74	72	73	74	74	74	74	74

It produced much nausea and sickness, with a great disposition to vomit : at ten minutes the pulse was fuller than natural ; the nausea continued for a considerable length of time.

EXPERIMENT IX.

At half after ten in the morning, I took 70 drops of the tincture with proof spirit, Mr. Merry attending to the pulse, beating 76 in a minute.

Minutes.	5	10	15	20	25	30	35	40	45	50	55	60
Pulse.	78	79	78	78	79	78	78	77	76	76	75	76

In ten minutes the pulse was fuller than natural; it exerted very sensible effects on the head, and caused some drowsiness.

EXPERIMENT X.

At 8 o'clock, A. M. Mr. Merry took 8 grains of the gummi-resinous extract, dissolved in proof spirit, his pulse beating 58 in a minute.

Minutes.	5	10	15	20	25	30	35	40	45	50	55	60
Pulse.	60	61	63	63	60	58	58	59	58	58	58	58

In five minutes the pulse became much fuller and stronger, and continued so till the 25th minute, after which it resumed its natural fulness and force. At 15 there was some pain and tension in the head. At twenty-five there was an increased perspiration. It acted very evidently as a diuretic.

EXPERIMENT XI.

At 9 o'clock in the evening I took ten grains of the gum dissolved in water; Mr. Mitchell attending to the pulse, beating 76 in a minute.

Minutes.	5	10	15	20	25	30	35	40	45	50	55	60
Pulse.	77	78	78	77	76	76	76	76	76	76	76	76

The pulse was very little altered in fulness and force. It excited some nausea, which soon went off.

EXPERIMENT XII.

In order to ascertain the effect of the lupulus on the bowels, when taken for some time, I took sixty drops of the tincture, every evening, for one week, and found that it had very little effect on them; but if any thing, it rendered them more laxative.

Two of my friends were so polite as to become the subjects of the same experiment, and with nearly a similar result.

From these experiments, it appears, that the powder acts very considerably as a cathartic, but exerts very little influence on the pulse; this I attributed to the distressing nausea which it induced.

The tincture made with the highly rectified spirits of wine, is the most agreeable preparation, and acts more powerfully as an anodyne than any other, and should always be preferred when this effect is desired.

The infusion and decoction are both very disagreeable, and are much less active than the tinctures.

The resin is more active than the gum, which explains the greater activity of the tincture, than either the infusion or the decoction.

From the preceding experiments with the tincture, and many more which I have thought it unnecessary to relate, it appears, that it acts decidedly as a powerful narcotic, and has this very great advantage, that it either renders the bowels laxative, or leaves them in the same state, in which they were when the medicine was exhibited.

ON THE MEDICAL PROPERTIES OF THE LUPULUS COMMUNIS.

The Hop was introduced into the *materia medica*, at an early period, and received, like many other medicinal articles, all those extravagant encomiums, which the enthusiastic physician is so much disposed to heap on a medicine that he has employed with advantage.

The advocates of different theories ascribed to this vegetable, different virtues, in order to explain its *modus operandi*, agreeably to the principles they had respectively espoused, as 'a sweetner of the blood,' as an 'antiseptic,' as an 'attenuant,' &c. But these erroneous epithets are now justly discarded; and the fanciful theories which gave them birth, have vanished before the rays of a better light, and will soon be consigned to oblivion.

The hop has either been mentioned very superficially, or passed over entirely, by a majority of modern writers on the

materia medica. But this should excite no prejudice against it; for many of the most celebrated and useful medicines of the present day, have, in their turn, experienced the same fate.

Dr. Darwin and Professor Barton,* have, placed the hop in that extensive and important series of medicines, called *Sorbentia* by the former, and *Bitter Tonics* by the latter.

From the preceding experiments, I am inclined to think it intitled to a respectable station on the list. I will first speak of its utility in

CALCULUS.

The opinions relative to the exhibition of the hop, as a remedy in, and as a preventive against the formation of, calculous, are directly opposite; some asserting that the number of calculous cases has augmented: while others contend that they have diminished since the introduction of that article into malt-liquors.

Mr. Ray, who is entitled to much attention, says that calculous cases have become much less frequent in the city of London, since the use of malt-liquors has become more general; and it appears, from the London bills of mortality, that the disease of calculous has by no means encreased, and that it is not a frequent one in that city, where the use of malt-liquors is so general.

Dr. Sydenham complains of the frequency of calculous symptoms in his time; and observes, that they occurred among his gouty patients, in the decline of life. Here this illustrious physician takes notice of the great analogy that subsists between the two diseases, and that gouty patients were very apt to become calculous in the decline of life. But to speak more properly, gout and calculous are the same disease, differing only in situation. They are both the effect of morbid excitement, or wrong action; and both consist in a calculous effusion; the one in the joints, the other in the urinary organs. The mere difference of situation cannot be said to constitute a difference in the *nature* of them,

* M. S. Lectures on *Materia Medica*,

From the circumstance of gout being so frequently the effect of the *immoderate* use of ardent spirits, and from calculus being so often the consequence or concomitant of gout; we are induced to ascribe Dr. Sydenham's increased number of calculous cases, to the alcohol, and not to the hop.

A Cyprianus, a writer of high authority, asserts, that he performed the operation of lithotomy, on 1400 patients, and that not one of them was a drinker of malt-liquors; but that many of them were wine-drinkers. This fact related by a man of such extensive observation, is entitled to peculiar attention; for it seems to prove, in a very impressive manner, that the drinking of malt-liquors, seldom, if ever, contributes to the production of calculus.

Dr. Lobb makes the following extravagant assertion respecting the virtues of the lupulus, viz. That the decoction of the plant had, in less than three days dissolved a very hard calculus; and that it is one of the more powerful lithontriptics. This assertion I have not thought of sufficient consequence to induce me to put to the test of actual experiment; but admitting that the decoction had the power of dissolving a calculus out of the body, it would by no means follow that it could effect the same in the body, after having undergone the course of the circulation. I wish I could, with safety, bestow upon the lupulus, or any other medicine, the appellation of a *lithontriptic*, but I fear that many years must elapse before mankind will be blessed with so precious and invaluable a discovery.

Although I cannot ascribe to any medicine the power of *dissolving* a calculus, I will readily admit, that certain medicines may prevent the peculiar secretion on which the formation of calculus depends. This secretion is frequently the effect of debility; and the hop, by acting as a tonic, may remove this state of the system, and, of course the further formation will cease.

Experience has taught us, that many of the most efficacious medicines, in procuring relief from calculus, are derived from the class of bitters; and I should suppose the hop possessed of that power in a very eminent degree; for, besides its tonic powers, it has the advantage of operating on

the urinary organs, in increasing the flow of urine: and perhaps, all medicines which have that effect, are of more or less utility in this disease.

It would be as tedious as useless, for me to go through the long catalogue of diseases in which the hop might be used with advantage, as a tonic. The actual state of the system, which is so strongly inculcated, in his lectures, by our professor of the *materia medica*, should ever direct us in all our prescriptions; and all diseases arising from a defect of action, in which the union of an agreeable bitter and tonic is indicated, cannot fail of being benefited by it.

Having finished the consideration of the hop as a tonic, I proceed, in the next place, to speak of its virtues as a narcotic. This I deem its most valuable property.

I cannot admit, with our illustrious Professor of the Institutes, &c. that the induction of sleep consists merely in elevating or depressing the system, to what he calls the 'sleeping point;' for facts compel us to admit, that some substances are endowed with a soporific quality, and that some possess the power of inducing sleep much sooner than others; however inadequate our present knowledge may be to the explanation.

The narcotic effects of the odor of the hop, are generally known; and it is very common to send to the brewers to get hops to use in this way. I have heard of several instances, in which the 'hop-pillow' has succeeded in procuring placid and refreshing sleep; and it is said, that this kind of pillow produced sleep, during the illness of the present king of England, after his physicians had prescribed every thing else in vain.

From the odoriferous quality of the hop, I should readily conceive that sleep might be induced by means of the pillow, particularly when we consider, that the patient, from the vicinity of the hop to the respiratory organs, must necessarily be constantly inhaling the odors. We have also seen, that its properties are easily volatilized by a moderate degree of heat, and that its narcotic effects could not be perceived, in any of the preceding experiments, when heat had been applied.

From these facts, we must conclude, that the *principium narcoticum*, whatever its precise nature may be, is volatile ; and that the odor of the hop, will produce the effects of the plant, when used in this way.

The narcotic effects of porter, and other potations, in which the hop is an ingredient, are so evident to all who use them, that they have universally obtained the epithet of ' heavy drinks.' That the anodyne effects of these liquors are derived from the hop, is proven by their being proportionate to the quantity of the hop, which enters into the composition : for instance, beer exerts its anodyne effects more decidedly in the summer, than in the winter ; because the brewers find it necessary to use more of the hop for its preservation, in the former season, than in the latter.

Dr. Barton informs me, that he has himself repeatedly taken the tincture of the hop, made with the highly rectified spirits of wine, and that he has frequently prescribed it to his patients. He says it has generally, if not always, seemed to induce evident narcotic effects, similar to those of opium. In himself, in twice the dose of laudanum, it induced sleep as decidedly as laudanum. The Doctor also observes, that it agrees with patients in whom laudanum, or opium in any shape, produces sickness at stomach, head-ach, and other disagreeable effects. He does not assert that it never induces head-ach ; on the contrary, he is persuaded that it sometimes does : but he has not known a single case of its producing the disagreeable *pruritus*, or itching, which is so frequently the effect of opium.

I will relate the case of a lady, which Dr. Barton has communicated to me.

Her situation often rendered it necessary for the doctor to prescribe laudanum for her ; but the disagreeable effects, such as head-ache and sickness at stomach, which it produced, even in very moderate doses, obliged him to have recourse to some other medicine. The tincture of the hop, in the dose of forty drops, induced profound and placid sleep, and none of the above disagreeable effects. This was the case on more than one occasion. Dr. A. Fothergill, in a

paper published in the *Philadelphia Medical and Physical Journal*, says he has found the hop possessed of an anodyne power, without the disagreeable effects of opium, and that it can be given for any length of time with impunity, whereas the Portland powder, and other bitters, if long continued, seldom fail of producing the most alarming consequences.

I will mention in this respectable physician's own words, the cases in which he has found it of most advantage.

'The cases in which I have found it to succeed best, have been such as demanded a light, agreeable bitter, combined with an anodyne quality: as, First—in various cases of dyspepsia, attended with pain and flatulency in the stomach and bowels; entire loss of appetite, and restless nights. Secondly—in catarrhs and asthmas, and other cases attended with a troublesome tickling cough, and great inquietude. Thirdly—in painful cases of gravel and stone, or biliary concretions, or severe pains of child-bed women. Lastly—in the above and other painful cases, where an opiate was greatly wanted, but could not be exhibited in any of the usual forms, without producing violent retching, severe head-ache, or other very untoward symptoms. Here a strong infusion of the hop, pursued freely, both internally and externally, has seldom failed to soothe the pains and finally, to procure a calm, tranquil sleep.'

Dr. De Roches relates, several cases in which the hop had succeeded in procuring sleep, after opium had failed.

From the recommendations of Professor Barton, in his lectures, the following gentlemen were induced to prescribe it, and they have been so polite as to communicate to me the result.

Mr. Parish related to me the case of a patient, in which it was necessary to exhibit something to procure rest, and opium always produced very disagreeable effects; he then had recourse to the tincture of the hop, which induced all the good, without any of the disagreeable effects of the opium.

Mr. Mitchell informed me, that he has frequently, during the last summer, prescribed the tincture of the hop, as a

substitute for laudanum, and that it induced the anodyne effects of the laudanum without any of its disagreeable consequences, such as sickness at stomach, costiveness, &c. In order to make the trial fairer, he gave them other medicines for laudanum, but was unable to procure rest by any of them except the hop.

I gave to a child, that could not rest without laudanum, some of the tincture of the hop, and it induced sleep, equally well with the laudanum.

I was informed, that a gentleman who had been in the habit of using laudanum, wished to discontinue it, but was unable to sleep without it; he had recourse to the tincture of hop as a substitute, with the effect of inducing sleep.

The utility of a medicine, endowed with the divine power of inducing sleep, must be as obvious to the humane physician, who tenderly feels for the distresses of his patient, as advantageous to the unfortunate sufferer, labouring under disease. Nothing, indeed, can give the physician, a more exalted idea of his profession, than the knowledge of being possessed of a medicine, capable of allaying the pains of his patient, and inducing a calm and tranquil sleep.

Opium, the medicine generally resorted to for this purpose, was thought of so much importance, that it attained the appellation of the *magnum Dei donum*. But however justly entitled this celebrated medicine may be to the highest commendations, many cases must occur to every practitioner, in which its exhibition is either useless, or totally impracticable.

1. Opium, administered in every shape, often fails of inducing narcotic effect. We have seen, in the preceding cases, that the hop has here succeeded. It must be highly consolatory to the physician, to know that he can have recourse to the latter, after the failure of the former.

2. Opium frequently disagrees so much with the constitution, that it cannot be exhibited with advantage. In such cases, the Hop has not disagreed with the patient, and has induced the desired effects.

3. Opium is often rendered inadmissible, from its constipating effects. In this case, the hop must be an excellent substitute; for, from the preceding experiments, it evidently appears, that it rather augments, than diminishes the alvine excretions.

AN INAUGURAL ESSAY

ON

WOUNDS OF THE INTESTINES.

SUBMITTED TO THE EXAMINATION OF THE

REVEREND JOHN ANDREWS, D. D. PROVOST
PRO TEMPORE:

THE TRUSTEES AND MEDICAL PROFESSORS OF THE UNIVERSITY
OF PENNSYLVANIA,

ON THE FIFTH DAY OF JUNE, A. D. ONE THOUSAND EIGHT
HUNDRED AND FIVE.

FOR THE DEGREE OF DOCTOR OF MEDICINE,

BY THOMAS SMITH, OF THE ISLAND OF ST.
CROIX:

HONORARY MEMBER OF THE PHILADELPHIA MEDICAL
SOCIETY.

.....*ut si*

Caecus iter monstrare velit.....*Hor*

INTRODUCTION.

IT is proposed in the following pages to take a brief view of the different methods which have been recommended for treating wounds of the intestines, to describe certain experiments, on brute animals which were made to ascertain the method most likely to prove successful, and to offer some doubts relative to the common opinions on this subject. I do not intend to enter minutely into the general mode of treating wounds of the intestines, but to confine myself entirely to the best manner of stitching them. Perhaps there are few accidents, to which surgeons are called, where they find themselves more at a loss how to proceed, than in wounds of the intestines. These circumstances, as well as the frequent fatality of such injuries, evince the great importance of the subject. It is well known to surgeons, that the most trifling puncture, made into the cavity of the abdomen, is apt to induce very serious consequences, from the tendency which the peritonæum has to inflame, when slightly injured; how much must the danger be increased when an intestine is wounded, and an opening produced, through which its contents may pass into the cavity of the abdomen. We are, however, told by a celebrated author,* that there is very little to be apprehended from this circumstance, on account of the equable pressure which is always kept up in the abdomen by the vis-

* Mr. John Bell.

cera. But I hope to prove from experiment, that his ideas were not altogether correct on that subject. The invaluable work on hernia, of Mr. Astley Cooper, gave rise to this essay, and the ingenuous observations of Messrs. Cooper and Thompson, respecting the difference between the consequences of longitudinal and transverse wounds of the intestines, induced me to attend particularly to that part of the subject.

INAUGURAL ESSAY.

WOUNDS of the intestines may be known by a passage of blood from the mouth and annus, as well as by the discharge of feces and fœtid air from the external wound, and they ought to be suspected, when nausea, vomiting, violent griping, pains through the abdomen, cold sweats or faintings occur after, penetrating wounds of that cavity. The intestines are sometimes wounded without protruding through the external wound: in such cases it would be of very little advantage to know, whether the wound was transverse or oblique; for the method to be pursued must be similar to that in simple penetrating wounds of the abdomen, viz. blood-letting and a low diet. Some authors recommend dilating the external wound, and searching for the injured bowel; but the danger arising from penetrating wounds in the abdomen, of all sizes, is so great, that in no instance ought it to be attempted, as there are cases on record of persons recovering from a wounded bowel, without having been under surgical treatment. It is therefore, only in cases, where the wounded intestine is protruded that the suture can be properly applied. The different kinds of sutures which have been recommended, have all had their advocates; the most ancient, and that which appears to have been most generally used is termed the glover's suture, which I shall now take the liberty of describing. In making this suture, a fine small round needle should be used armed with a silk thread, which has been previously waxed. The surgeon bringing the lips of the wound in contact, perforates both edges at the same time, and carrying the needle to the same side at which it entered, he must make a second

stitch, at a small distance from the first, perhaps the eighth of an inch, and in the same manner by a proper number of stitches, must close the wound throughout its whole extent. This being done, a sufficient length of thread is left out at the external wound for the purpose of drawing it away, when we suppose the wound of the intestine to be united, which is generally completed in six or seven days : in withdrawing the ligature, care should be taken to do it very gently, least we should destroy the adhesions which have taken place, This mode of stitching a wounded intestine, is certainly a very complicated process, and should be dispensed with in every instance for a more simple one.

A more modern method has been spoken of by Mr. Ledran, which is termed the looped suture. To make this suture, an assistant takes hold of one end of the wound whilst the surgeon does the same with the other, and the needles, which should be round, straight and small, carrying each of them a thread a foot long, and not waxed, must be equal in number to the stitches intended to be made : as many threads are now to be passed through both lips of the wound as are thought necessary, taking care that they are a quarter of an inch distant from each other. All the threads being passed, the needles are to be withdrawn, and the ends of the threads on each side are tied, after which, joining them together they are twisted into a sort of cord : by this means, the divided portions of intestines are drawn into pleats, so that the stitches which were distant about a quarter of an inch are now brought together, and thus the lips of the wound are prevented from separating. The ligatures are to be fastened to the external dressing, afterwards they remain until the wound in the intestines is healed ; they are then to be untwisted, and all the ends cut off on one side ; after which they must be withdrawn slowly and separately. The same objection may be applied here as in the glovers suture, this is certainly a more complicated process, and it increases the danger of the operation, by lessening the diameter of the intestine, thereby occasioning dangerous obstructions.

Mr. John Bell has recommended in wounds of the intestines, that we should only use one single stitch, which should

be passed through the wounded bowel, and then drawn through the integuments of the abdomen at the external wound. But notwithstanding what Mr. Bell has said, of the equable pressure which is kept up among the viscera, preventing the fæces from being shed into the cavity of the abdomen; I must, however, beg leave to differ from him, for in the experiments which I performed, I found that treating the intestine in this manner was not sufficiently secure for preventing the fæces from escaping into the abdominal cavity.

The following suture has also been proposed in a complete division of the intestine *. It was first recommended by Mr. Ramdhor in cases of hernia, where a portion of the intestine had been destroyed by mortification. In these cases, he has advised *to extirpate the diseased part*, and to introduce the upper portion of the sound intestine within the lower, for about an inch, and to confine it there by sewing it once or twice round with a fine needle and thread; but besides the difficulty of knowing which is the upper or lower portion in wounds of the intestines. I find that it never can be performed on the living subject, as will appear by the ninth experiment; for immediately upon making the section of the intestine, the divided parts became so much inverted, as to render the introduction of one within the other, utterly impossible. The method which appears to promise most success, is that recommended by Mr. Astley Cooper, in his work on hernia, in that part wherein he treats particularly of mortification of the intestine. He directs, that the injured part should be removed, and the divided portions brought into contact, and secured by four stitches, one being at, or near the mesentery, and the others at equal distances from each other.

This method is certainly the most safe and simple of any that has yet been spoken of, and will no doubt in time be generally adopted. Perhaps four or five stitches will be found sufficient in most instances of a complete division of the intestine. But we should be careful not to use more than are really necessary, for it has been observed, that puncturing the

* Mr. Benjamin Bell speaks highly of it in his *System of Surgery*.

intestine frequently, increases the danger of the operation very much.

In order to decide between these different methods, I determined to institute a series of experiments upon dogs by wounding their intestines, and uniting them with the various sutures above stated. I am aware that it is not easy to determine with precision the treatment proper for the human species, by inferences derived from the dog; but the analogy in the present instance appears to me very strong. I shall in the next place commence, by relating my experiments.

EXPERIMENT I.

APRIL 7.

Assisted by my friend Mr. Klapp, an incision was made into the abdomen of a dog, and one of the small intestines; having been brought into view, a transverse section was made into it, and the wound secured by four stitches, one at the mesentery, and the other three at equal distances from each other, the threads were then cut off at the knots, and the external wound closed by the interrupted suture.* The animal did not appear to have suffered materially from the operation, for in twenty four hours he took food, and after the first day exhibited no symptoms of indisposition. On the 30th, he was killed, the wound of the intestine was found completely healed; the place at which the intestine had been divided, appeared somewhat thickened, considerable adhesions were observed among the small intestines. Three of the ligatures had disappeared, the other was still remaining loosely attached to the internal coat, and probably would have been discharged in the same manner as the others, had the dog been permitted to live a few days longer.

* The interrupted suture was used in every instance, for securing the external wound, unless particularly mentioned.

EXPERIMENT II.

APRIL 7.

On another dog, I repeated the same operation, with this difference, that the ligatures which had been cut off in the preceding experiment at the intestine, were now left out at the external wound, in case it should be necessary to withdraw them. In consequence of the restlessness of the animal during the operation, considerable violence was done to the parts, before they could be reduced. On the second day after the operation, the dog appeared so ill as to make his recovery doubtful: On the 4th day, it was thought proper to remove the ligatures; after this he appeared better and took nourishment. On the 19th, he was killed: upon laying open the abdomen, the effects of inflammation were still obvious. The omentum was found adhering to the parietes of the abdomen, and very much indurated. Preternatural adhesions had taken place among all the viscera, but more particularly in the small intestines, which were knotted and twisted together in an astonishing manner. The intestine at the place where the wound had been made was not quite united.

EXPERIMENT III.

APRIL 9.

Several of my friends honored me with their attendance, whilst the following experiments were performed; the abdomen of a dog having been opened, and the small intestines brought into view, a longitudinal incision of about an inch and a half was made parallel with the mesentery, which was secured by four stitches, and the intermediate spaces sewn with a fine thread to prevent the fæces from escaping into the cavity of the abdomen:* the threads were cut off at the intestine. The animal died in about thirty six hours. On dissection the marks of inflammation were found much less than might have been expected.

* According to the proposal of Thompson of Edinburgh.

The wound in the intestine was completely torn open, excepting at one stitch.

EXPERIMENT IV.

APRIL 9.

A full grown dog was submitted to the same experiment as the former, with this difference, that the intermediate spaces between each stitch were left unsewn. Six hours after the operation, the animal vomited stercoraceous matter appeared dull and drowsy. On the 10th, in consequence of food being offered, of which he took a small quantity, vomiting was again excited. On the 12th, he took food, and from that time appeared to be doing well. Seven days after he was killed; on opening the abdomen the omentum was found adhering to the site of the external wound, being considerably indurated. The mesenteric glands were enlarged. The wound in the intestine was not completely united, two of the ligatures had disappeared. The other two still remained; the wounded gut had adhered to the mesentery and adjoining portion of intestine.

EXPERIMENT V.

APRIL 10.

On a full grown tarrier, I repeated the former experiment, wishing to see, whether a longitudinal incision could not by great care and attention, be so managed, as to do away the opinion of its being universally fatal. To effect which, a very small opening was made through the parietes of the abdomen, and a portion of intestine, being brought into view, it was divided longitudinally for about two inches, and afterwards secured by six stitches which were cut off at the knots. The parts having been returned, the lips of the external wound were brought together and secured by adhæsiue plaister*. The animal did not appear to have suffered in the least from

* For it was observed, that the ligature used for securing the external wound increased the inflammation very much.

the operation, for in less than twenty-four hours he took food and has continued doing well ever since.

EXPERIMENT VI.

APRIL 16.

Pleased with my success in the preceding experiment, I obtained another dog and opened his abdomen. In one of the small intestines. I made a longitudinal wound for about three inches, and treated it in every respect similar to that related above. This animal appeared to have suffered very little more than the other, considering the extent of the wound, for in about twenty-eight hours he eat and continued doing so until the tenth day after the operation, when he refused nourishment. Two days after he died, on examination, it was found that the wound had healed completely, but directly above the wound a bone half an inch long, and nearly as broad, was discovered to have perforated the intestine*.

EXPERIMENT VII.

APRIL 16.

Wishing to know how much of the intestine might be removed, without much endangering the life of the animal, I performed the following experiment: having obtained a full grown dog, an incision was made into the cavity of the abdomen, two inches of one of the small intestines were removed, the divided portions were then brought together, and the wound was treated as the transverse incisions had been. In dissecting of the divided portion of intestine, some of the branches of the mesenteric arteries were wounded, but did not bleed during the operation. On visiting him in the afternoon, I found there had been a considerable hemorrhage which still continued. I did not open the wound, but applied a piece of wetted linen to the parts, which had the desired effect. On the

* This must have been owing to a diminution of the intestinal canal which is always produced by longitudinal wounds of the intestines.

18th, the belly being somewhat tense, two of the external ligatures were cut away, that the blood, should any have collected, might be discharged; but the wound did not open, and the dog soon resumed the appearance of perfect health, which continued without interruption until May 6, when he was killed. The divided portions of intestine were found united, and the ligatures had been all discharged*.

EXPERIMENT VIII.

APRIL 16.

Having opened the abdomen of a pointer pup, three inches of intestine were excised, the arteries being secured, the intestine in other respects, was treated as the last had been. In twenty minutes after the operation, he vomited the food which he had taken in the morning, and appeared dull the remaining part of the day. Three days after the operation, he took food, and continued doing well. May 6th, he was killed, and the abdomen being opened, it was with difficulty I could ascertain where the division had been; the coats of the intestine appeared somewhat thickened; one of the ligatures remained attached internally.

EXPERIMENT IX.

APRIL 18.

Having divided the intestine of a dog transversely, I attempted to treat it in the manner spoken of by Mr. Ramdohr, viz. by introducing the upper extremity of the divided intestine within the lower; after having procured a piece of candle, as directed by him, it was inserted into that portion of intestine, which was supposed to be the uppermost. I then endeavoured to introduce the superior within the inferior, but the extremities of each became so inverted, that it was found utterly impossible to succeed, it was therefore given up and treated in the way recommended by Mr. John Bell, using only one

* The viscera in this experiment appeared much more natural than in any other, probably from the hemorrhage that took place, which shews the propriety of bleeding largely in such cases.

stitch, and fastening it to the parietes of the abdomen. The dog took food the day after. On the 20th, it was observed that the fæces were discharging at the external wound, when the animal appeared very weak, but still continued to take food. On the 21st, he was much worse, and the abdomen being tense, the ligatures at the external wound were removed to facilitate the discharge of the fæces which gave a temporary relief. On the 22nd, he died. On examination there was found a considerable quantity of fæces and water in the abdominal cavity. One portion of the intestine had united to the external wound through which part of the fæces were discharged.

EXPERIMENT X. & XI.

APRIL 28.

Wishing to give Mr. John Bell's method of stitching an intestine a fair trial, I made the following experiments: having obtained two full grown dogs, a transverse incision was made into the intestines of each of them, which was secured by one stitch and fastened to the wound. No. 10, died in about twenty-four hours. The marks of inflammation were very great, and the fæces had been discharged into the abdomen. No. 11, died on the 2nd of May. The intestines appeared very much inflamed, fæces as in the other instances were found in the abdomen, also water which the animal had drank. The large intestines appeared gangrenous and tore very easily.

EXPERIMENT XII.

A pointer pup of about two months old was submitted to the following experiment: a triangular piece was cut out of one of the small intestines, and the wounded intestine sewn to the parietes of the abdomen. The animal very soon showed symptoms of indiposition and died in thirty hours. On examination, the peritonæum and all the viscera of the abdomen were found considerably inflamed, a quantity of water was also in the cavity.

It appears then from the result of my experiments on dogs, that not only the intestine may be returned into the cavity of the abdomen, but that the ligatures may be cut off and returned with the intestine,* and that we need not be under any apprehension of their being discharged into the cavity, for by some process of the animal economy of which we are ignorant, the ligatures have in every instance either been discharged with the fæces or been found loosely attached to the internal coat of the intestine. It has been said by Messrs. Cooper and Thompson, that there is a curious difference in the facility with which a longitudinal and transverse wound of the intestine unites. But in all the experiments which I have made, it was found that with care the longitudinal united as kindly as the transverse, only requiring a little more attention to the diet of the animal, which should be very sparing and liquid until the wound has had time to heal. It certainly requires more pains to close a longitudinal wound of the intestine completely, than one which is transverse. The longitudinal incision always occasions a diminution in the diameter of the intestinal canal, thereby producing dangerous obstructions. If it should be of any considerable extent, probably the surgeon would be justified in cutting out the wounded portion and treating it as a transverse division. This may be done without much endangering the life of the animal, as appears by two experiments where three inches of the intestine were removed.

* As was observed by Mr. Thompson of Edinburgh.

A CHEMICO-PHYSIOLOGICAL

ESSAY,

DISPROVING THE EXISTENCE OF AN ÆRIFORM FUNCTION IN
THE SKIN, AND POINTING OUT,

BY EXPERIMENT,

THE IMPROPRIETY OF ASCRIBING ABSORPTION TO THE
EXTERNAL SURFACE

OF THE HUMAN BODY.

SUBMITTED TO THE EXAMINATION OF THE

REVEREND JOHN ANDREWS, D. D. PROVOST
PRO TEMPORE:

THE TRUSTEES AND MEDICAL PROFESSORS OF THE UNIVERSITY
OF PENNSYLVANIA,

ON THE THIRD DAY OF JUNE, A. D. ONE THOUSAND EIGHT
HUNDRED AND FIVE.

FOR THE DEGREE OF DOCTOR OF MEDICINE,

BY JOSEPH KLAPP, OF ALBANY, NEW-YORK:

HONORARY MEMBER OF THE PHILADELPHIA MEDICAL
SOCIETY.

‘Fiat Experimentum.’

To Mr. JOSEPH KLAPP,

Dear Sir,

IT afforded me great satisfaction to witness the zeal and industry with which you conducted the enquiry which has led to the important deductions contained in your Inaugural Dissertation, disproving the existence of an æriform function in the skin.

With pleasure I bear testimony to the neatness and accuracy with which your experiments were performed, and consider them as completely satisfactory on this interesting subject.

That the light which you have thrown on this question may stimulate you to extend your enquiries to other branches of the science of medicine, and that you may enjoy health and happiness in life, and meet with success in the exercise of your profession, is the ardent wish of

Dear Sir,

Your sincere and affectionate friend,

JAMES WOODHOUSE.

May 3d, 1805.

INTRODUCTION.

A favourite author, not less distinguished for literary accomplishments, than correctness of observation, says, a composition that enters the world with a view of improving it, has a claim to the utmost indulgence, though it fail of the effect intended. If such encouraging lenity await the voluntary writer, whose object to instruct is often alloyed by an indignant thirst for public adulation, my claims on the score of indulgence cannot but meet with liberal success, as nothing less urgent than a law of the University could have induced me, at this juvenile period of my life, to appear in the character of an author.

The subject which has been selected for this essay is one which has been much canvassed by men of acknowledged talents; many of the difficulties, however, attending its satisfactory investigation, were either left neglected, or imperfectly surmounted, though it for a while attracted the experimental attention of a Cruikshank, a Priestley, and an Abernethy.

The present enlightened state of medical science excepts none from the privilege of laboring in her enchanting dominion; and as invaluable truth is alike appreciated, whether detected by the searching penetration of irresistible genius, or discovered by the good fortune of a mere novice in medicine, I have been induced to venture on the subject under consideration, without pausing to offer an apology for interfering with the labours and opinions of such eminent characters.

.....and truth alone,
Shall be our chosen theme, our glory to the last.

COWPER.

The theoretical and practical advantages derived from a correct knowledge of the laws and operations of the animal body, are too generally valued, to require commendation, and the pernicious tendency of ascribing functions to parts not recognised by the economy of nature must be evident to every reader.

When we recollect that just ideas of the nature and cure of disease, cannot be obtained without the aid of physiological instruction, the necessity of cultivating this important branch of our science with nicety and carefulness will occur to the most of observants, and the often unfortunate consequences of mistaken publications cannot be exemplified in a more striking manner than a candid disclosure of the error in which I was nearly involved.

The transpiration and absorption of the gases by the skin, as apparently demonstrated by authors of unquestionable integrity and reputation in medicine, have for some time past engaged my consideration, and so firmly did I believe in their proofs and observations, that a series of experiments were actually instituted to ascertain the influence and connection of these functions with the pulmonary forms of fever. But the result of a few of their most interesting experiments carefully repeated, gave me reasons to believe that there was some fallacy in the case, and a further enquiry, which was then anxiously carried on, soon confirmed my suspicions.

The experiments which constitute the basis of this essay were conducted without any preconceived notions which could bias their natural import, and the fidelity with which they are detailed, can be attested by a few ingenious friends who had the goodness to assist me in their performance: but how far they will go to effect a decision on the subject, I leave to be determined, with all due deference, to the better judgment of those who may hereafter give them an attentive examination.

INAUGURAL ESSAY.

PART I.

Physiologists have long laboured with diligence to discover the functions of the skin.

The insensible emanations from its surface were known to Galen ; and the great Sanctorius proved, in his statical experiments, that they consisted of watery vapour, endowed with certain excrementitious qualities.

Since the days, however, of these two celebrated physicians, new researches have been made, and many of which have ended in imputing to this organ a more important office in the animal body.

Count De Milly recorded a paper in the memoirs of the Royal Academy of Sciences, at Berlin, in the year 1777, in which he adduces experiments in proof of the discovery of an elastic fluid which passes off by the skin. The imperfect manner in which experimental inquiries were conducted at this period of physiological improvements, necessarily connected with chemical knowledge, gives many reasons for believing that the count was mistaken ; particularly, when it appears, from the experiments of a judicious and more modern writer*, that no correct inference can be drawn, of the uniform function of the skin from the analysis of gases collected, unless they were first received over quicksilver. Count De Milly says, being in a warm bath, half a pint of air might have been collected over the water, in the space of three hours ; and from his analysis, which was both inaccurate and very incomplete, says an author † in Nicholson's Journal, he concluded it was carbonic acid gas.

* Abernethy.

† Trousset.

Two years subsequent to the publication of the above memoir, Mr. Cruikshank, an ingenious physiologist of England, gave to the world an essay on insensible perspiration. His object being chiefly to investigate the quality of the watery vapour emitted by the skin, much information on the æriform discharge could not be expected.

The following experiment, however, I will take the liberty of detailing, as the only one, in his publication, that relates to our subject. He introduced his foot, previously washed and dried, into an empty bottle, excluding the access of external air by means of a moistened bladder; after retaining it in this situation for one hour, he withdrew it, 'the fluid collected,' says he, 'produced no change on the lime water, but lime water thrown into the bottle and agitated became as turbid as when the air in which the wax taper had extinguished itself, was mixed with it. 'This last experiment, continues Mr. Cruikshank, 'I repeated several times and with the same success; from this I inferred, that admitting the common theory of fixed air and phlogistin, something passed off with the vapour of insensible perspiration of the skin which rendered air fixed.' Inductions from incorrect data have always been fruitful sources of error in medical philosophy. This experiment, for two good reasons, cannot authorise the conclusion which Mr. Cruikshank has deduced; first, because a moistened bladder will, by furnishing carbon to the oxygenous portion of the air in the bottle, yield fixed air; and secondly, because lime water poured out of one bottle into another, with the addition of agitation, will become turbid, from the chemical union of the carbonic acid of the atmosphere with the calcareous earth, as any one will find by making the experiment.

Doctor Ingenhouz, about this time, paid some attention to the nature of the bubbles of air which he saw arise from the skin while immersed in water; but his experiments are few, and are liable to many objections that apply to De Milly's and others who have not taken the necessary precaution of ascertaining the nature of the gases contained in the water in which their experiments were made.

Mr. Troussset, a French physiologist, has published an account of his experiments made in water, in which the transpiration of carbonic acid gas is denied, but the emission of azotic air by the skin is contended for.

That gases are disengaged from the surface of the body, while immersed in common water, no one will pretend to deny; but that they are prepared in the body, and emitted by minute vessels which open on the skin, is fully refuted by all the experiments detailed in the sequel of this essay. If then, the air collected, does not come from the vessels of the skin, we must look to the water as the only source which can, under these circumstances, yield it.

As plants, under the influence of solar light, will readily disengage from pump-water, and the most of other waters in which they are placed, a considerable quantity of pure air, and as Professor Woodhouse has clearly proved, from a number of decisive experiments, that it is produced by the decomposition of carbonic acid with which the water is impregnated, I may conclude that the generality of waters contain fixed air; and that waters are more or less impregnated with azotic air, according to local circumstances, is now too well ascertained, to be disputed by any one. With these considerations, founded upon incontrovertible truth, I do not hesitate to determine, in my own mind, that the evolved heat of the hand, foot, or of the whole body, which were the subjects of experiments under the direction of different physiologists, expanded the gases contained in the water which they used, and thus caused them to rise in bubbles of air from the surface of the skin. This conclusion is rendered more probable, when we recollect the experiments which have long since been made by Dr. Priestley in water exhausted of all its air, in which not a single bubble of any gas whatever was seen to arise, though the skin was immersed in it for a considerable time.

The Doctor expressly declares, that the perspiration of animals does not contaminate common air as the process of respiration does. I am well aware it is asserted that water absorbs carbonic acid as fast as it is emitted by the pores of the skin, and that this circumstance accounts for the result of the experiments alluded to. But to this I ask leave to reply, in the

first place, that fixed air does not so rapidly unite with water as to preclude an opportunity of detecting it, if any were thrown out; and in the second place, that this objection does not apply to this mode of experimenting to discover whether azotic air is ever thrown out in perspiration, as many physiologists assert; and as they have erred in one instance, it is more than probable, from what has already been advanced, that neither carbonic acid nor azotic air, is ever thrown out in the perspiration of animals. But what renders the explanation which I have given, of the origin of the gases collected over water, in which the hand or the whole body were immersed, still more satisfactory, is the simple, though not less conclusive experiment which was made, by holding my hand and forearm, [previously invested by varnished silk which was impervious to air] in a tub of pump water: in a short time innumerable sphericles of air were seen both by my friend and myself, to form on the external surface of the silk, and gradually rise to the top of the water.

The successive formation and disengagement of minute bubbles of air, being always confined to the surface of the skin, while immersed in water, and not proceeding from portions of water remote from its surface, may be urged as an argument in favor of gaseous transpiration; but error, from this quarter, would be too palpable, to elude detection; vegetables, while immersed in water impregnated with carbonic acid, and exposed to the light of the sun, disengage bubbles of pure air from the surface of their leaves; yet who will contend that it comes from their fibrous texture? In my humble opinion, the disengaged heat of the skin enables us to explain, as clearly, the one fact, as the decomposition of fixed air does that of the other.

The embarrassment in which many Physiologists have involved themselves, on the interesting subject of the functions of the skin, I regard as similar, in many of its concomitants, to the once prevalent, though erroneous opinion, that vegetables purify atmospheric air, by absorbing its azotic, and perspiring its oxygenous portions. This opinion, however, by the light of modern discoveries, has been exploded, as an unhappy hypothesis, originating out of incomplete researches;

and for an explanation of the source of the air disengaged by healthy plants, under water, we are very justly referred to the previous condition of this fluid.

Mr. Abernethy, a practical physiologist, and surgeon, of London, published in the year 1793, an essay on the functions of the skin, in which he maintains, by a variety of experiments, that not only fixed air and azotic gas are thrown out by the skin; but that oxygen air and carbonic acid gas are very readily imbibed; whilst the nitrous, hydrogenous and nitrogenous gases tardily gain admittance into the absorbing vessels. I do not think it necessary to detail, in particular, the experiments which Mr. Abernethy performed, but will mention that the chief of them were made by holding his hand and wrist in an inverted glass vessel, and in a medium of different gases confined over mercury. He also made two or three experiments in water, but in these he could not discover that carbonic acid gas was emitted by the skin; believing that the water absorbed the gas, he soon discontinued this mode of prosecuting his subject.

M. Seguin, in 1792, delivered a paper on the functions of the skin, to the Royal Academy of Sciences, at Paris. The accuracy and precision with which this philosopher conducted his experiments, have never been questioned; yet it is well known, that they afford a result entirely different from those of Mr. Abernethy. M. Seguin, satisfactorily proved, in his paper, by a great number of ingenious experiments, that the skin, with its cuticle entire, absorbed neither air nor water.

Mr. Fourcroy declares, that ‘it is not true that there pass off, through the skin, as some moderns have asserted, elastic fluids, and especially carbonic acid gas.’

After having adduced the above respectable authority in behalf of the sentiments which are laid down in this essay, I will first venture a few observations, founded on anatomical and physiological knowledge, pointing out the improbability of an æriform function in the skin, and then proceed to detail the experiments which fully authorise my disbelief in such an operation. In the very commencement of this undertaking, I am met by a conviction of being incapable of rendering that justice to truth, which is requisite for its permanent establish-

ment in the records of medical science ; but, as the maxim of the late illustrious and unfortunate Lavoisier, is deeply engraven upon my mind, I feel but little apprehension of being deluded from the proper track, by the fascinating incentives of hypothesis.

‘ We ought, in every instance, to submit our reasoning to the test of experiment, and never to search for truth, but by the natural road of experiment and observation.’ *Lavoisier*.

When assertions are made, either of a practical or theoretical nature, from a set of experiments, or a train of plausible reasoning, if, upon a close examination, they are found not to accord with the experiments of others, well conducted, and more coincident with the economy and operations of the animal body, it has long been a standing rule in medical philosophy, to consign them to merited oblivion.

On physiological subjects, correct anatomy will always be regarded as the polar star to successful researches ; and when she has performed her duty, the established laws of the animal body, like the compass which guides the ship at sea, ought only to be entrusted with the important office of conducting us to the end of our journey.

Mr. Abernethy says, ‘ the removal of a quantity of oxygenous gas, from common air, is surely a curious circumstance ; if this be the effect of an action in the absorbing vessels, it must much exalt our ideas of their subtilty, and their aptitude or disposition to admit one species of matter, and to reject another. That the absorption of one air, in preference to another, depends upon this cause, I believe will not, upon reflection, be doubted. It might indeed be suspected, that oxygenous gas was separated from the atmosphere, by the skin, as it is in the lungs, by chemical attraction : but it has been proved, that carbonic acid is removed with equal celerity ; and experiments on animal substances shew, in them, a disposition rather to part with, than to imbibe, carbonic acid. The removal of this air is, therefore, not likely to be the effect of chemical affinity.’

My objections to this reasoning of Mr. Abernethy are, first, his data are far from being established ; on the contrary, the repetition of his chief experiments in my hands, fully

disprove the existence of a gaseous function in the skin. The highly ingenious researches of Seguin, before mentioned, have given conclusions directly opposed to those of Mr. Abernethy; but they correspond with the result of my enquiries. In the second place, lymphatic absorbents cannot be satisfactorily demonstrated on the skin, though the best dissections have not been wanting, to ascertain their presence. The science of Physiology only embraces the healthy actions and powers of parts, the existence of which have been previously known to the anatomist; all beyond these prescribed limits must be uncertain, if not entirely conjectural. But if oxygenous, carbonic and other gases are taken up by the skin, and carried into the system, why have they not been detected, either in their gaseous form, or incorporated with the fluid circulating in the absorbing vessels?

The next question that naturally occurs, is, how are these gases disposed of, admitting for a moment that they are actually taken in by the lymphatic vessels of the skin? As Mr. Abernethy has excluded chemical attraction from any agency in the case, they must be conducted through the course of the absorbents to the subclavian veins. The quantity of oxygen gas absorbed from the surface of the whole body, must be considerable, if the absorbents of the hand and wrist are capable of imbibing eight ounces in eight hours, as Mr. Abernethy asserts; would not then the blood in the left subclavian vein, which receives the thoracic duct, be of a more florid color than that of the right, which only receives the lymphatics of the right arm, and of the right side of the neck? And lastly, if oxygen gas be absorbed by the skin, and conducted through the course of the lymphatic vessels into the general circulation may we not call in question the economy of the animal body as the lungs appear fully competent to all the purposes of oxygenating the system.

A Chemist of this city, whose judgement I have reason to place great confidence in, informed me, that if gases were given out by the skin, they might be discovered by holding the arm in a glass vessel filled and inverted in water. As I had ascertained the nature and proportions of the gases con-

tained in the pump water at the Pennsylvania Hospital, where some of my experiments were made, no doubt was entertained but that some information might be derived from this mode of experimenting. Accordingly, a few experiments were made in this manner; as water, however, has been objected to, I will not relate them in particular, but deem it only necessary to mention that the gases collected, differed in no considerable degree from those which had been previously obtained from the water.

As mercury has never been objected to, as a proper medium to experiment in, for accomplishing the objects of our present inquiry, a sufficient quantity was procured, and a tub arranged with necessary conveniences to contain it, as I was fully persuaded, that if any airs were thrown out by the skin, they could, under the circumstances of the following experiments, be easily detected.

EXPERIMENT 1. *Thermometer 73°.*

A glass vessel being filled with, and inverted in quicksilver, all air that adhered to my hand and wrist was carefully separated by moving them in different directions, under the surface of the quicksilver for ten minutes; they were then introduced into the inverted vessel and there retained for more than two hours; for a few minutes, in the beginning of the experiment, my hand was in some degree benumbed with cold, but its usual warmth soon returned, and at the expiration of the time mentioned, little or no inconvenience was experienced, but what arose from the pressure of the mercury. During all this time, not a bubble of either carbonic acid or azotic air, was seen to emanate from the skin. This experiment was made eight months ago, and has since been repeated in the presence of Professor Woodhouse, with precisely the same result.

As lime water has always been regarded as a correct test for carbonic acid, it appeared plain, that if fixed air was thrown out by the skin, it would, while the hand and wrist were immersed in it, form, by chemical combination, carbonat of lime,

and the milky colour produced would afford a good criterion to judge of its emission. Under this impression the following experiment was made.

EXPERIMENT 2. *Thermometer 70°.*

My hand and wrist were introduced into a jar containing lime water, whose purity was previously ascertained, and held in this situation for one hour; during this time no carbonat of lime was formed, but on the contrary, the lime water, on withdrawing my hand, was as transparent as it was previously to its use. This experiment was repeated at another time, with no other variation than that of its being continued two hours, the result of which was the same as in the first; so that no deception could have occurred in its performance.

Mr. Abernethy, firmly believing that elastic gases were emitted by the skin, objected to water as a medium to experiment in, to discover this function, on account of its great disposition to absorb carbonic acid; all experiments, therefore, made in this manner, must be incomplete as relating to this acid gas, unless the state of the water be attended to, subsequent to their performance. With this consideration the following experiment was instituted.

EXPERIMENT 3. *Thermometer 58°.*

Having half filled a convenient vessel with water, that had been previously boiled for some time, in order to separate all loose air that it might be impregnated with; my foot and ankle were immersed in it, and retained in this situation for three hours, and then withdrawn. My object was now to ascertain, whether or not the water had imbibed carbonic acid; to accomplish which, a glass vessel was filled and inverted in the same water, a small handful of healthy leaves, of a species of geranium, were introduced into the inverted vessel, and the whole exposed to the rays of the sun for four hours, during which time, no oxygen air was disengaged from the water. In this experiment, we may with safety conclude, that carbonic acid was not contained in the water, and consequently none was emitted by the skin; for it has been proved to a certainty, that healthy leaves of plants will readily decompose carbonic

acid, if contained in the water in which they are immersed, while in the light of the sun; its coal will be devoured for food, and its oxygen escape in the form of pure air.

Lest a few of those who may honor this dissertation with a perusal, should hesitate in admitting experiment 1, as satisfactory, on account of the pressure of the quicksilver, the following one was made, which proves, beyond a doubt, that carbonic acid gas is not emitted in the perspiration of animals.

EXPERIMENT 4. *Thermometer 56°.*

Having procured some pure hydrogen air, from diluted sulphuric acid, and the filings of malleable iron, four ounce measures of it, were thrown up into a glass vessel, previously filled and inverted in quicksilver: my hand and wrist, after all loose air was separated from their surface, as in experiment 1, were introduced into the inverted vessel, and kept in this situation, for three hours. My hand and wrist being surrounded by the hydrogen gas, in this experiment, suffered nothing from either the pressure or coldness of the quicksilver. Professor Woodhouse observed this, as well as the chief experiments detailed in this essay; and in his presence, the air in the vessel from which my hand and wrist had just been removed, was examined. Having ascertained that the volume of air had not been in any degree, diminished, we passed up one ounce measure of it, over lime water, in an eudiometer; no milky appearance was observed by either of us: about three ounce measures of lime water were then passed up into the vessel containing the remainder of the air in which the experiment was conducted; but no carbonic acid could be discerned, in either case.

Dr. Rush relates, in his lectures, an experiment which induces him to believe in an æriform function of the skin: a lighted candle, he says, in the morning, was extinguished by the air of the bed in which he had slept, the preceding evening. I do not hesitate to admit the experiment, as correct, but cannot subscribe to the inference of our ingenious Professor.

The readiness with which respiration renders air impure, from the consumption of its oxygenous portion, is well known to every physiologist, and in my humble opinion, the conta-

mination of the air in the bed, was owing to this process. This explanation will appear more probable, when we consider how often we lay, in the course of an evening's sleep, with our heads under the bed clothes.

Having discovered, by all the above experiments, that the skin, with its membranes entire, has no connection with the office of transpiring elastic fluids, I will next relate those experiments, which, in my own mind, undeniably prove, that gases are not absorbed from the external surface of the human body.

EXPERIMENT 5. *Thermometer 60°.*

Five ounce measures of atmospheric air were thrown up into a glass vessel, previously filled and inverted in quicksilver; my hand and wrist, with the necessary precautions used in experiments 1 and 4, were introduced into it, and retained in this situation, with little or no inconvenience from the mercury, for three hours, and then withdrawn. We immediately examined the air in the vessel; its volume was not diminished, and, when passed through lime water, no milky colour ensued; which was an additional proof that the skin did not throw out carbonic acid; we next proceeded to ascertain whether or not any of the oxygenous portion of the air had been removed during the experiment; for this purpose, one measure of it was passed up, over water, in an eudiometer charged with phosphorus, and in less than twenty-four hours, the absorption of $\frac{22}{100}$ was complete. In this experiment, no addition or alteration occurred in the atmospheric air to which my hand and wrist had been exposed for three hours; but with the view of giving complete satisfaction on the subject, the following experiment was performed.

EXPERIMENT. 6. *Thermometer 54°.*

Five ounce measures of oxygen air, of 4 per cent purity, being transmitted into an inverted vessel, previously filled with quicksilver, my hand and wrist, freed from all adhering air under the surface of the mercury, were introduced into it, and held in this situation three hours. Upon examination, the

quantity of air had suffered no diminution; it was then passed through lime water, and as turpidness did not occur, we inferred that carbonic acid had not been given out by the skin; the air was next tested, as in the former experiment, in the eudiometer of Berthollet, and was found not in the least adulterated.

The experiments just related, prove, in the first place, that carbonic acid is not transpired; in the second place, that azotic air is neither emitted nor absorbed; and in the third place, that oxygen air is not inhaled by the skin of the human body. The care with which they were conducted, and the completeness of their plan, exclude all the deception which experimental enquiries, in the hands of inattentive observers, are often liable to.

In order to discover whether, or not, fixed air be removed from the external surface, by cutaneous obsorbents, the following experiment was made.

EXPERIMENT 7. *Thermometer 61°.*

Five ounce measures of carbonic acid obtained from carbonat of lime, by diluted sulphuric acid, were transmitted into a glass vessel, previously filled and inverted in quicksilver; my hand and wrist were then introduced into it, and kept in this situation for three hours. The volume of air was next attended to, in order to accomplish the object of the experiment; it was found diminished in quantity only about half a drachm. The removal of which, I do not hesitate to ascribe to the combination of the carbonic acid with the perspirable fluid; as my hand, when removed from the inverted vessel was considerably moistened by this discharge. When we call to mind the affinity that naturally subsists between aqueous substances and this acid gas, the effect which we have mentioned is what ought to be expected: besides, it is highly improbable that the skin, even if it were endowed with the power of absorption, would inhale carbonic acid which is thrown out by the lungs, as, excrementitious from the system, and refuse to absorb oxygen air, confessedly the pabulum vitæ, as was the case in experiments 5th and 6th.

The experiments are now related, which have created, in my own mind, a firm belief, that a few physiologists for whose talents in the science of medicine, I shall still cherish the highest sentiments of respect, have, from the illusion of unforeseen circumstances, which often render of no avail the best directed enquiries, erroneously imputed an æriform function to the skin.

It is to Mr. Abernethy, whose pre-eminent abilities and industry have long entitled him to the first rank in his profession, that we are indebted for the best mode heretofore used in the investigation of our subject. But while I am recording the merit which so justly belongs to this distinguished character, perhaps the zealous friends, and cultivators of Physiology will regret, that his ingenious researches had not terminated in the acquisition of truth. Their motives on this interesting occasion would, I am conscious, have sufficient grounds of support; for one great source of ignorance and uncertainty in medicine is the accumulation of experiments and plausible theories, which are directly opposed to each other.

In taking leave of this part of my essay, it will not be amiss to observe, that the preceding, as well as the following pages, are respectfully submitted to the examination of the public, not with the expectation of dislodging, immediately, opinions heretofore credited, but solely with the hope of calling the attention of others, who may ingenuously decide on the subject of the controversy; as I shall be satisfied, either in being supported or refuted, by such an interference.

EXPERIMENTS,
AND
OBSERVATIONS ON THE IMPROPRIETY
OF ASCRIBING ABSORPTION TO
THE EXTERNAL SURFACE OF THE HUMAN BODY.

PART SECOND.

When first the duty devolved on me to write an Inaugural Dissertation, I intended to have confined myself to the consideration of those subjects which are above treated of, but as time was not wanting, and the usual limits allotted to a publication of this kind, were not exceeded, I resolved to extend my enquiries still further into the functions of the skin.

Physicians, both in ancient and in modern times, have been at a loss to account for the manner in which many medicines produce their respective effects upon remote parts of the animal body, when only applied to the external surface.

Some have supposed that this phenomenon in medicine, could not be satisfactorily explained, without admitting, that a portion of the articles were taken up by the absorbing vessels, of the skin, and conveyed into the general circulation, to be directed to their appropriate parts, and there give rise to effects corresponding with the nature of the medicines employed.

In behalf of this theory, we have on record the works of many respectable physicians, among which none are so eminently entitled to our consideration as the experiments and observations of Dr. Alexander Monro.* The doctor made a great variety of experiments on frogs, in different conditions, all of which tend to prove that, in them, opium, ardent spirits, and essential oils are absorbed from the external surface. He found that after the crural nerves of one frog were cut, and

* Physical Essays, Vol. 3.

the hind half of the spinal marrow of two others completely destroyed ; camphor applied to the hind legs of all, produced the same effects, in nearly the same time, as when applied to a sound animal. ‘But to prove this absorption in fact,’ says our learned author, ‘I divided two frogs at the pelvis two hours after the camphor had been applied to them in the above way ; I then pulled the skin off the fore part of their body ; and found, that the flesh and bowels had a smell of the camphor. To discover this more certainly, I cut them in pieces, and poured on one, rectified spirit of wine and on the other, water : and was sensible of the taste of the camphor, both in the spirit of wine, and in the water.’ As all the experiments made by the Edinburgh Professor, were conducted without any regard to the lungs, which I shall soon prove to be an extensive apparatus of absorption, I do not hesitate to pronounce them incomplete, as the articles which he used might have passed into the system through this quarter, and not absorbed from the external surface, as he inferred. But even if I was disposed to allow that his experiments completely prove that absorption does take place from the surfaces of frogs, they cannot, with any certainty be used in the investigation of our subject. An amphibious animal is widely different, in its anatomical structure, way of life and its economy, from man. If then, it should hereafter be satisfactorily ascertained, by a series of new experiments, or by a repetition of those of the Professor, with a proper variation, that the frog is endowed with the function of cutaneous absorption, the discovery would not, in my mind, excite surprise, even if I should establish, by the experiments shortly to be detailed, that this office is unconnected with the economy of the human body.

Dr. Barton, who perhaps is better acquainted with the functions of the lymphatick system, than any other man in America, is inclined to give some credit to cutaneous absorption in frogs. He informed me in private conversation that he had frequently observed, that if this animal was confined in a dry glass vessel, it became enfeebled, diminished in its natural size, and scarcely able to leap ; but if a small quantity of water was poured into the vessel, or the air in it only loaded

with moisture, it soon acquired its wonted vigour, its body became plump, and its motions were usually lively. In the first part of the sixth volume of the Philosophical Transactions an account is given of the *Lacerta Subviolacea*,* in which it is mentioned, that this animal was weighed at different times. On the 24th of March it weighed 342 grains, but in somewhat less than an hour, it weighed only 324 grains, having lost 18 grains, 'It is a well ascertained fact, however,' says the ingenuous naturalist, Dr. Barton, 'that the weight of many of the Amphibia, particularly the frogs and lizards, is very various at different times, even in the course of the same day or hour. This difference of weight is often entirely independent on any aliment, whether solid or fluid, being taken into the stomach, and must be ascribed to the absorption of water.'

Dr. Darwin, the ingenuous author of the *Zoonomia*, says, 'that those who have remained half an hour in a warm bath, when they have previously been exhausted by exercise, or abstinence from food or fluids, have absorbed so much as to increase their weight considerably. Dr. Jurin found an increase of weight to 18 ounces, by sleeping in a cool room, after a day's exercise and abstinence; so much, in that situation, was absorbed from the atmosphere.'†

After having read the fair and conclusive experiments of Dr. Currie, the assertion of Dr. Darwin will appear highly erroneous; for in one instance, where death was caused by inanition, no cutaneous absorption occurred, though the patient was immersed in a warm bath for an hour. In Dr. Jurin's experiment, the absorption doubtless took place from the lungs, and not from the external surface.

It is argued in favor of absorption, that animals live in hot, moist climates, without drink, and yet discharge a considerable quantity of humors, both by perspiration and urine; and that in some diseases of the human body, a much greater quantity of urine is discharged, than the quantity of drink taken in. That animals do live in hot, moist climates, without drink, we will readily admit, but that their thirst is allay-

* A species of Lizard first described by Dr. Barton, and called by him *Lacerta Subviolacea*.

† *Zoonomia*, page 440,

ed, their cutaneous and urinary discharges carried on, in consequence of absorption by the skin, is an induction which modern discoveries will not countenance; for doubtless, in them, as in cases of hydropic and other diseases, where a much greater quantity of urine is often discharged than the quantity of drink taken in, the lungs supply the exigencies of the system. And what more particularly renders my reasoning at least plausible, on this subject, is the experience of a number of respectable physicians.

Dr. Rollo* published an account of an interesting case of diabetes, in which he mentions that the weight of the patient was not increased by a continuance of ten minutes, in a bath of 110 degrees of heat. 'In the year 1788, Dr. Currie happening to be at Buxton, made an experiment on the effects of bathing, on the weight of the body. After half an hour's immersion in the bath, he found his weight rather diminished than increased. In the year 1790, he had a patient in diabetes, whose cuticle, as is usual in that disease, was in a morbid state. Being desirous of trying how far the inordinate action of the kidneys might be affected by a gentle stimulus applied to the skin, he immersed him in a bath of the temperature of 96 degrees, weighing him before and after the immersion. In this case, no variation in the weight could be detected. Dr. Currie afterwards made five different experiments of the same kind, upon himself, varying the heat of the bath, from 87 to 95 degrees; but he could never, in any one instance, find his weight augmented. It may however be said, that though in diabetes, where the epidermis is diseased, no liquid is inhaled; and though in health, when the vessels are full, no absorption may take place; yet, when the body is wasted, from a want of proper food through the stomach, the plastic powers of nature may be employed to supply the defect, and to excite an inhalation through those pores on the surface, by which an exhalation is usually performed. To prove that this does not happen, Dr. Currie relates, very minutely, a remarkable case of dysphagia, where death was the consequence of inanition, notwithstanding every attempt

* Zoonomia, page 255.

to support the system, both by the rectum, and by the surface. This patient, on different occasions, stepped perfectly naked, upon Merlin's balance, immediately before immersion; and again immediately after it, his body being previously dried. The weights were never moved. The result was surprising; for Dr. Currie could not distinguish the slightest variation in the weight of the body, though the beam would have detected a single drachm, though the immersion had been continued for an hour, and though a constant friction had been kept up, nearly the whole time, on the inner surface of the thighs, with the view of encreasing the action of the absorbents. If the non-absorption by the surface of the body be established, it will, Dr. Currie observes, ascertain, that, in the ordinary course of things, contagion is received into the system, by the lungs only, and will, he thinks, justify a practice, which he has been informed, is common among our more experienced seamen, on the coast of Guinea, and other warm climates, who when exposed, during the night, to a breeze from the marshes wrapt their heads in a sea-cloak, or other covering, and sleep fearless on the deck, with the rest of their bodies nearly exposed.'

Since the celebrated French Philosopher, Bichat, suggested that absorption took place from the lungs, and published the experiments corroborative of his opinion, we have not been at a loss to account for the manner in which certain volatile articles applied to the skin, have, in a short time passed into the system, and manifested their presence by the effects which they produce on the excretions of the body. The following quotation from this author, will afford the reader an epitome of his theory.

Extract from a work entitled '*Recherches Physiologiques sur la Vie et la Mort* : par XAV. BICHAT, Professeur d'Anatomie et de Physiologie. A Paris, an. 8.' Page 352. 'La respiration d'un air chargé des exhalaisons qui s'élèvent de l'huile de thérébentine, donne aux urines une odeur particulière. C'est ainsi que le séjour dans une chambre nouvellement vernissée influe d'une manière si remarquable sur ce fluide. Dans ce cas, c'est bien évidemment par le poulmon, au moins en partie, que le principe odorant passe dans le

sang, pour se porter de là sur le rein; en effet, je me suis plusieurs fois assuré, qu'en respirant dans un grand bocal, et au moyen d'un tube, l'air chargé de ce principe qui ne sauroit alors agir sur la surface cutanée; l'odeur de l'urine est toujours notablement changée.' 'The respiration of an atmosphere charged with the vapours of spirit of turpentine, gives the urine a peculiar odour. Thus, after remaining some time in a chamber newly painted, we find that a remarkable change is produced in this fluid. In this case, it is extremely evident, that, through the lungs, at least in part, the odorous principle passes into the blood, and from thence to the kidneys. In fact, I have frequently convinced myself of it, by breathing air charged with this principle, through a tube adapted to a large glass vessel, (by which means the cutaneous surface of the body could not be acted on) and always found the odour of the urine remarkably changed.'

In this experiment, as the vapour was not applied to the surface, it could only act upon the lungs.

The experiments and observations of the Parisian Professor, are fully established in an inaugural essay by Doctor Rosseau, an ingenious French physician, of this city *.

As many articles, however, occasionally applied to the skin, for medicinal, and other purposes, are not endowed with an odour sufficiently volatile to gain access to the lungs, so as to effect any remarkable change in the system, the operation which they produce, must, therefore, be referred to the reciprocal harmony which subsists between the various parts in the organized body. Tartrite of antimony, rubbed upon the skin, excites nausea and vomiting; which are the usual symptoms produced when it is taken into the stomach †.

A poultice of tobacco leaves applied to the region of the stomach, excites an emetic operation, after large doses of sulphat of zinc have been given without producing this effect. ‡

* Dr. Rosseau made his experiments in this city, about the same time that professor Bichat published his Physiological work, in Paris.

† Dr. Barton.

‡ Sherwin's Experiments have since been repeated by a graduate of this university, and found correct.

Opium produces sleep, when externally applied ; and in certain affections of the stomach, many physicians, and particularly Dr. Whytt, have availed themselves of the knowledge of this circumstance with manifest advantage to their patients.

Dr. Barton informed me that he had cured several cases of intermittent fever, by applying poultices of the Cinchona bark, to the skin. These, and other active articles, in my humble opinion, produce their respective effects on the animal body, through the medium of sympathy ; as the theory which supposes their absorption, is involved in too many difficulties to be generally received.

I am well aware that Professor Monro has made experiments which may induce some to believe that opium acts chiefly upon the system, after being applied to the skin, in consequence of its absorption. But I have before observed that the results of experiments made upon frogs, cannot, with any propriety be contrasted with those of experiments made on the human body.

Having now premised the facts and observations which are necessarily connected with the investigation of my subject, I will next proceed to lay before the reader the experiments which demonstrate, in the first place, that the lungs furnish an extensive surface for absorption ; and in the second place, that the skin of the human body has no claim to that function.

The important experiments of Bichat, and those made in this city by Dr. Rosseau, have certainly given an extensive range to our views of the Physiology of the animal body ; but as the emanations from spirits of turpentine which they breathed, might have been conveyed either in part, or wholly, into the system, by the absorbing vessels of the mouth, fauces and trachea, over which parts this active article passed before it could have come in contact with the lungs, their experiments, in my opinion, do but incompletely prove what they have adduced.

With the intention, however, of ascertaining this subject more certainly, I made the two following experiments : for

assistance in their performancē, I am indebted to my worthy friends and fellow graduates, Messrs. Smith and Legaré.

EXPERIMENT I.

A dog was secured upon a table, by passing leather straps, in different directions, around his body and extremities. An incision was made through the skin and muscles, just above the upper end of the sternum; and the trachea was laid bare; a strong ligature was passed around it so as to intercept all communication between the lungs and the mouth of the animal. A longitudinal opening was then made, immediately below the ligature, into the trachea, to which was adapted one end of a long tube, the other end being passed out of a window. In this situation the dog breathed the air of the atmosphere, through the tube, for more than two hours, during which time his mouth, fauces and trachea above the ligature, were frequently inflated with the vapours of spirit of turpentine. Now it is evident, that if absorption exclusively belongs to the lungs, under the circumstances of this experiment, the vapours of spirit of turpentine cannot be conveyed into the system. At the expiration of the time mentioned, the dog was carried into another room, the tube was removed, and the ligature which had been around the trachea was divided by the scalpel, and withdrawn; the animal after I had stitched up the aperture from which the tube had just been removed, breathed in the natural way. In this condition he was left from 11 o'clock in the morning, until 6 in the evening, when his existence was instantly terminated, by thrusting a knife between the superior cervical vertebræ into the spinal marrow, with the view of procuring from his bladder the urine which had been secreted in the course of the day, it being retained by a ligature which I had passed around the penis, in the beginning of the experiment. After the urine was collected in a cup, Mr. Legaré and myself examined it with great attention, but neither of us were able to detect in it the least smell of violets.

The result of this experiment convinced me, that though the enquiries of Bichat and Rousseau had been incompletely

conducted, yet the inference which others deduced from them was an important truth. But what establishes, beyond a doubt, the absorption from the lungs, is the following experiment.

EXPERIMENT II.

A strong dog being fastened down upon a table, an incision as in the former experiment, was made, just above the superior extremity of the sternum, and at this place, a ligature was passed around the trachea, which completely prevented the animal from respiring through his mouth; a small aperture was then made, below the ligature, into the wind-pipe, to which was adapted one end of a long tube, while the other end communicated with the mouth of a bottle containing spirits of turpentine. In this situation, the dog continued to inhale, by his lungs, emanations of turpentine, for two hours, the operation was then discontinued, the tube was removed together with the ligature about the trachea, and the incisions which had been made were closed. The mouth, fauces and trachea, of course, resumed their former functions. The dog was now left, from 11 o'clock, A. M. to half after six in the evening, when he was killed: the urine collected gave a strong smell of violets. My obliging friend Mr. Thomas Smith, and myself, made ourselves acquainted with this circumstance from repeated examinations.

The above two experiments, will, if I am not mistaken, convince the candid reader that the lungs afford an extensive surface for absorption, and at the same time prepare him to expect the manner in which I shall prove that certain articles applied to the skin, gain admission into the system.

As most of the experiments made by different physiologists, in proof of absorption from the external surface, are exceptionable on account of their not having been conducted in such a manner as to exclude the lungs from an agency in the case; it occurred to me, that experiments made over quicksilver, in inverted vessels, would be void of such, or any other objection. As spirit of turpentine readily insinuates itself into

the system, and gives the urine a peculiar odour, which is a good criterion of its presence, I determined to make use of it, as the principle article in my experiments ; accordingly in

EXPERIMENT III.

My foot was placed in a wash-bowl, containing spirit of turpentine ; friction was occasionally used, and at the expiration of one hour, as I experienced some pain from the irritation of the turpentine, my foot was withdrawn. In a short time I retired from my room, and did not return until the expiration of one hour, when the urine was found to be impregnated with the smell of violets ; the odour, however, was much more powerful at subsequent examinations. In the course of half an hour, from the beginning of my exposure to the emanations of turpentine, a slight head-ache, with an evident acceleration of the pulse, were excited, which continued for several hours : but the urine continued to emit the smell of violets until the next day.

In this experiment, it is very clear that the spirit of turpentine was conveyed into the system ; but to ascertain whether the absorption took place from the skin or the lungs, the two following experiments were made.

EXPERIMENT IV.

Three days after performing the above experiment, a quantity of good spirit of turpentine was passed up into a glass vessel, previously filled and inverted in quicksilver. The cork in the mouth of the phial containing the turpentine, was drawn, under the surface of the quicksilver, so that the vapours of this volatile liquid, could not mix with the air of the room. My hand and wrist were next introduced into the inverted vessel, and in this situation, surrounded by spirit of turpentine, were retained for an hour and a half. My hand being immediately well washed, I left the laboratory and walked to a considerable distance ; in the course of an hour, the urine was attended to ; but the smell of violets was not in the least observable : the same reference was made repeatedly

during the remainder of the day, without being able to detect any change in the natural smell of the urine. The systematic affection which was experienced in the former experiment, did not occur in this. I continued to inspect the urine, occasionally discharged, until the next day, but nothing characteristic of the presence of spirits of turpentine was detected.

This experiment was conducted under circumstances which allowed every opportunity for absorption to take place ; friction was repeatedly used by rubbing my hand and wrist against the sides of the vessel which confined the turpentine over mercury ; and what I deem of great importance is the complete manner in which the lungs were excluded from an interference which they would otherwise make in the experiment. Having now ascertained that in my first experiment the turpentine which was evidently carried into the system, did not gain access through the agency of cutaneous absorbents, it was natural for me to discover whether the lungs, which with the skin were the only parts exposed to its emanations, had not been the organs which accomplished the absorption.

EXPERIMENT V.

A glass vessel, containing a quantity of atmospheric air, was inverted in quicksilver ; three or four ounces of spirits of turpentine were introduced into it, and agitated with the air contained in the vessel, in such a manner as to intimately mix the odour with every part of it. A convenient glass tube was then used, one end of which communicated with the air in the vessel, and the other end was taken into my mouth, and in this manner I inhaled the air highly charged with turpentine, without suffering any of it to come in contact with the skin. Having effected this part of my experiment, I left the room and took a moderate walk ; I had not retired long, before a slight head-ach and quickness of the pulse were experienced ; the urine voided, after an hour and a half had elapsed, was found imbued with the smell of violets ; but in the course of the afternoon, the impregnation was much greater, and which

continued to be observed until 11 o'clock, P. M. when I went to bed; the next morning the urine emitted only its usual smell.

The above experiments fully establish, in the first place, that spirit of turpentine is not absorbed from the external surface; and in the second place, when it manifests itself in the system by the effect which I have mentioned it produces in the urine, consequent to its application to the skin, or diffusion through the air, it has been absorbed by the lungs and not from the skin, as some physiologists have erroneously imagined.

It appears somewhat remarkable to me, that men most refined in their researches in medicine, and who have otherwise greatly enlarged our knowledge of the animal body, should attribute absorption to the skin, without extending their inquiries to collateral circumstances, which must infallibly end, either in refuting or establishing their doctrine.

If spirit of turpentine, garlic, camphor, asparagus, &c. and the gases, are taken up from the external surface, why have they not been found in some part or other, of the lymphatic system? In the most successful investigations that have been made, into the functions of the lacteal lymphatics, the nature of their contents has always been attended to, as affording a test of their having absorbed certain articles, previously presented to them. In answer to this, it may be said, that absorbents have not yet been discovered, opening on the skin of the human body, while lacteals have long since been traced by the anatomist. But this temporary evasion involves a more glaring error; for why contend, with so much assurance, for functions of parts whose existence is not yet proved? If the skin was endowed, as the lungs are, with the power of absorption, the extensive surface of these two organs, would be a constant source of disease in the system.

With a view to the general subject of absorption, and to ascertain, in particular, whether camphor is taken up by vessels of the skin, as Professor Monro contended for, in his experiments upon frogs, I made the following experiment.

EXPERIMENT VI.

Eight ounces of a strong solution of camphor, in spirit of wine, were transmitted, with all the necessary cautions of experiment 4th, into a glass vessel, previously filled and inverted in quicksilver; my hand and wrist were then introduced into the vessel, and retained in this situation, surrounded by the camphorated solution, for more than an hour. In the beginning of the experiment, 15 grains of nitrat of pot-ash were taken, with the intention of increasing the natural action of the kidneys, and thus affording a better opportunity of accomplishing the object in view. Friction, as in experiment 4th, was occasionally used, while my hand and wrist were held over mercury, in the solution of camphor. But, with all these measures to excite absorption, not a single circumstance occurred, either in the course of the day, or ensuing evening, which induced me to believe that this effect had taken place.

As it is a fact, familiar with the most of observers, that both the flesh and milk of animals which feed on garlic, are affected with the taste and smell of their aliment, I entertained no doubt, but that a strong infusion of this odorous substance, applied to the skin, under the circumstances of the following experiment, would throw some light upon cutaneous absorption taking the state of the urine and breath as a proper criterion.

EXPERIMENT VII.

A quantity of a strong infusion of garlic was made, a portion of which, with great care, was passed up into a glass vessel, previously filled and inverted in quicksilver: my hand and wrist were then introduced into it, and retained in this situation for one hour. The infusion was carried to the laboratory in an eight ounce phial, the cork of which was drawn, under the surface of the quicksilver, so that the air which I breathed was not in the least impregnated with it. Friction, as in the former instances, was frequently made use of. Upon withdrawing my hand from the inverted vessel, I instantly left the room, and having washed my hand with soap and water, I exercised myself in walking, for half an hour,

and returned home. The urine and breath were then referred to ; in neither of which, could the smell of garlic be perceived ; my attention to this part of the experiment was not here discontinued ; for examinations were repeatedly made, during the remainder of the day ; but the smell of garlic did not in any instance, occur.

The facility with which absorbing vessels of the alimentary canal take up garlic, is a convincing proof, that this substance is not unfriendly to the appetency of the lymphatic system. If then, the skin of the human body is endowed with the power of absorption, is it not probable, nay, I will venture to ask, is it not certain, that this diffusible substance would have been conveyed into the system, in the above experiment ?

As the presence of asparagus, in the system, can always be known, by attending to the state of the urine, I resolved to ascertain, whether or not, this article could excite absorption from the external surface.

EXPERIMENT VIII.

A quantity of a strong decoction of asparagus was, with necessary cautions to prevent its odour from passing into the air of the room, transmitted into a glass vessel, previously filled and inverted in quicksilver. My hand and wrist were next introduced into the inverted vessel, and kept in this situation surrounded by the decoction for more than an hour ; during the greater part of which time, friction was used, with the intention of giving every opportunity for an absorbing power on the skin, to exert itself. At the expiration of the time mentioned, my hand and wrist were withdrawn, when I directly left the laboratory, and in the course of one hour and a half, the urine was examined, but the peculiar odour which asparagus gives, could not be perceived. As in the former instances, I continued to attend to the urine occasionally discharged, during the remainder of the day ; but nothing was discovered which characterised the presence of asparagus in the body.

I have now completed the detail of the experiments and observations which have given rise to the opposition which I have here offered to the sentiments of many respectable physiologists, on the functions of the skin of the human body: I am, nevertheless, conscious of having left unrefuted, many arguments, drawn from analogical data, which, slender as they appear to me, may notwithstanding, tend to lessen that conviction in the minds of some individuals, which it has been my endeavour to produce.

An inquiry into the functions belonging to the external surface of the different orders of animals, will yet, I trust, though much light has already been shed upon this important subject, afford an interesting page in the annals of medicine.

I have for sometime past observed the want of such a work, and if circumstances do not mar my present expectations, I intend to devote a share of my future leisure to the collection of materials for its publication.

To Dr. Samuel Stringer, of Albany, I am much indebted for useful instruction conferred in the beginning of my studies; and in return for which he will please to accept of my thanks and unalterable esteem.

Before I close this imperfect essay, it remains for me to proffer my thanks to the Medical Professors of this University, for the permanent advantages which I have derived from their instructing lectures, and for the polite attention which they have individually manifested towards me. That they may long continue to exercise the duties attached to their respective departments, with the same reputation that has heretofore characterised their labours, is the sincere wish of their obliged and grateful pupil.

AN EXPERIMENTAL ESSAY
ON
CUTANEOUS ABSORPTION:

SUBMITTED TO THE EXAMINATION OF THE
REVEREND JOHN ANDREWS, D. D. PROVOST
PRO TEMPORE:

THE TRUSTEES AND MEDICAL PROFESSORS OF THE UNIVERSITY
OF PENNSYLVANIA,

ON THE FIFTH DAY OF JUNE, A. D. ONE THOUSAND EIGHT
HUNDRED AND FIVE.

FOR THE DEGREE OF DOCTOR OF MEDICINE,

BY HENRY P. DAINGERFIELD, OF FREDERICKS-
BURGH, VIRGINIA:

MEMBER OF THE PHILADELPHIA MEDICAL SOCIETY.

‘We ought, in every instance, to submit our reasoning to the
test of experiment, and never to search for truth but by the
natural road of experiment and observation.’

Lavoisier.

INAUGURAL ESSAY.

In selecting the subject of cuticular absorption for an Inaugural Dissertation, I have not been misled, either by the hope of offering any thing new on so interesting a question, or of rendering that which we already know more valuable by the beauties of my style, or the force and perspicuity of my reasoning.

Medical science is accused of having hitherto advanced with a slow and halting pace; and this too, perhaps, principally because it is an experimental science. Nature, although simple and determinate in her laws, nevertheless requires that he who consults her oracles, if he wishes to obtain correct responses, should undergo the fatigue of minute and laborious enquiry. It is this labour and fatigue that frequently determine physicians to remain in ignorance, or to be content with imperfect information. Instead of going to the fountain head, to ascertain whether the data from which they reason be founded in truth or in error, it is this pain and difficulty of making and of repeating experiments that determine them to receive theories as established, that are founded only on a single series of observations. It is in this way that one author has been induced to rely solely on another, and in this way that we have volumes on volumes that contain little else than a repetition of what had been previously known. In short until within the present enlightened era of philosophy, those that have experimented, have done it with so little attention to the minutiae by which the results of experiments are varied, have been so imperfect in the detail of their modes of

proceeding, that the mind is seldom able to rest satisfied with their conclusions and doubt and uncertainty have thereby been entailed upon us from century to century.

On the subject of cutaneous absorption, the medical world has not long been divided. From the times of Hippocrates and Galen down to those of Sabatier,* it has been received as an unquestionable fact, that substances are taken, by absorption, from the surface of the body into the general circulation. The contradiction of this doctrine, at once so novel, and apparently so absurd, could not fail to excite much attention. Much, therefore, has been said on both sides of the question, and physicians, once so unanimous in favour of such a power, are now, I believe, very much divided in relation to its existence.

Doctor Rousseau, a graduate in this university, was the first among us to institute a systematic examination of the question; and has, I think, by a set of ingenious and well-devised experiments, succeeded in proving, that there is no such function as cuticular absorption.

But for the establishment of so interesting a question, is it enough that we have only a single series of experiments? and those too supported only by the testimony of a single individual? may not the tests which he employed have been fallacious? may not the want of accuracy on the part of the experimenter have led to deductions widely different from truth? To decide, therefore, this point with accuracy, the only alternative left us is to repeat and extend his experiments. Should their results, when thus repeated, be strikingly coincident, surely we shall then have greater reason to rest satisfied with the truth of his opinions than we now have. Mine then, be the humble lot of such repetition; mine the satisfaction of contributing to render truth more certain, by multiplying the evidences on which it rests.

Upon superficial examination it might be imagined, that inquiries of this sort were better calculated to amuse than to

* A late French author, who, in conjunction with Lavoisier, wrote a paper on this subject for the Academy of Arts and Sciences at Paris.

furnish valuable practical information. But when we advert to the fact, that the assumption of the doctrine of cutaneous absorption has been employed to explain the origin of contagious and epidemical diseases ; the similarity of effect produced by the external application and the internal use of the same medicine ; and that on it has been predicated the use of numberless ointments and lotions ; we at once perceive the futility of such an objection : since a negative conclusion relative thereto would, on the one hand, free us from absurd theories ; and, on the other, save that time which would otherwise be wasted in an inert practice.

Although the errors into which physicians have fallen on this subject have been numerous, much may be said in extenuation of them. That phenomena should have been ascribed to the lymphatic system, in early times, which the better judgment of the present day has referred to a different origin, ceases to be a matter of astonishment, when we recollect, that this system of vessels is so minute, as to have eluded the observation of the best anatomists prior to the last fifty or sixty years. How then could it be expected that, within so short a lapse of time, the true character and functions of so large a portion of the animal economy, so minute in its parts and infinite in its ramifications, could be marked and defined without falling into error ! Men, too, who make discoveries in philosophy or the arts, are ever anxious to enhance their value, by ascribing to them every possible importance. Accordingly, no sooner was the existence of this system announced to the world, than it became necessary to assign to it uses commensurate with the ideas entertained of its value by its discoveries. Hence we find it made, not only the vehicle by which every digestible substance taken into the stomach is distributed throughout our bodies, but the medium by which every medicine, whether of internal or external application, produces its appropriate effects on the animal system.

The doctrine of absorption, as now taught, may perhaps be properly divided into lymphatic and lacteal. Of the truth of our opinions relative to the latter, as thus delineated by the elegant and learned author of the Botanic Garden, there can be no question.

' Thus where the veins their confluent branches bend,
 And milky eddies with the purple blend ;
 The chyle's white trunk, diverging from its source,
 Seeks through the vital mass its shining course ;
 O'er each red cell and tissued membrane spreads,
 In living net-work, all its branching threads ;
 Maze within maze its tortuous path pursues,
 Winds into glands inextricable clues ;
 Steals through the stomach's velvet sides, and sips
 The silver surges with a thousand lips.'

But when authors, proceeding still further, would explain the *modus operandi* of medicines, by conveying them unaltered into the general circulation, by means of absorption, we are compelled from facts to deny their assertions, and to adduce the experiments of Doctors Hodges and Walmsley * to prove that such is not the case in relation to medicines taken into the stomach ; and that it is not in relation to those applied to the skin, will, I hope, be no less satisfactorily demonstrated, in the sequel of this essay.

Before we proceed further, it may not be improper to pass in review some of the principal arguments in favour of absorption from the external surface. An attention to the perspirable matter with which our bodies are incessantly bedewed, appears first to have led modern physiologists to suspect the existence of a set of vessels appropriated to this effect ; and the subsequent discovery of the lymphatics, opening to their view the strong analogy between it and the lacteals, where, perhaps, they had already contemplated the function of absorption, determined them to convert this similitude into a further proof of the existence of cuticular absorption. Those vessels then, no sooner discovered than charged with the important office of absorption, have given birth to the theories already noticed ; and the skin, which would appear to have been formed as a covering to the more delicate parts of our system, has been made the ready inlet of a thousand predisposing and exciting causes of disease. The rapid diminution that succeeds the application of carbonic acid to the skin has

* See their Inaugural Dissertations.

been supposed to depend on its transmission through those vessels into the general circulation. The narrative of the unfortunate sailors, who resisted the dreadful effects of thirst, by wearing wet jackets; the presence of spirits of turpentine and garlic in our breath and urine, when we have been exposed to their emanations; and, above all, the fact that mercurial frictions salivate, have severally been adduced in proof of such transmission.

But let us return to the results of a more rigorous examination of facts. In reply to the first proposition, it may be affirmed, that positive experiment has proved the diminution of the carbonic acid to proceed from its absorption by the moisture of the hand, for which it has a very great affinity, and, consequently, that this phenomenon has no connection whatever with organic life. The second position is no less objectionable. Thirst doubtlessly depends upon a waste of fluids; a cold application, therefore, constringing the pores, checks perspiration, and thus mitigates its pains: not to add the insurmountable objection, that the absorption of a single drachm of water never could be detected by the best balance that ever was invented, although the body was subjected to long and repeated immersions in baths of every variety of temperature *. To the third it may be replied, that whenever spirits of turpentine or garlic, have passed into the general circulation, they may be decidedly proved to have entered it through the medium of the lungs; organs whose powers of absorption have been sufficiently demonstrated. And to the fourth and final proposition, it is also objected, that although it is difficult to assign a satisfactory theory of the mode in which mercurial frictions operate, yet it may be affirmed, from incontestible evidence, that the one now under consideration is perhaps as wide of the mark as any that could have been suggested.

Extending the principle of absorption from the lungs, as contended for by Seguin, Dr. Rousseau has attempted to explain their operation, by supposing the mercury to be volatilized during its application, and to be inhaled by the lungs.

* See Dr. Currie's elegant experiments on this subject, in his work on the hot and cold bath.

With this view, he has adduced numerous instances of persons who have been salivated, during their attendance on venereal wards, or after having been exposed to the influence of the more volatile saline preparations of this metal.

That the atmosphere of a room, in which a number of persons have been daily in the habit of using large quantities of different mercurial preparations, should become sufficiently impregnated with this fluid to excite salivation in their attendants, I can readily believe; nor do I find more difficulty in admitting a fact which is thus stated by Chaptal *: ‘Oxygenous gas, obtained from the mercurial oxides, almost always holds a small quantity of mercury in solution. I have been a witness to its having produced a speedy salivation, in two persons, who used it for disorders of the lungs. In consequence of these observations, I filled bottles with this gas, exposed them to an intense cold, and the sides became obscured with mercurial oxide, in a state of extreme division.’

Here, as in the first instance, we perceive the means by which mercury may be volatilized, and taken into the lungs. But when we consider the nature of mercurial frictions, this explanation certainly fails us; since it is neither essential to their operation that the patient should inhale oxygen gas, obtained as above described, nor that he should frequent a venereal ward. ‘May it not be supposed,’ it is contended, ‘that, during the frictions, some of the mercury is, by the action of the air, assisted by the heat of the body, oxyded, and that it afterwards parts by degrees with its oxygen, which carries along with it to the lungs some parts of the mercury, which is with difficulty separated from it,’ and thereby produces its specific effects?

In reply to this overstrained construction of possibilities, it may be affirmed, that positive experiment has proved, that neither the blue pill nor mercurial ointment are oxyded by friction. † Hence the doctor is deprived at once of the vehicle by which he conveys this fluid to its place of destination, and

* Vide his Elements of Chemistry.

† See Mr. J. Douglas’ Inaugural Dissertation.

of course of his theory. But even though we grant, for argument's sake, that mercury is oxyded in the case of which we speak: how does it happen that a few drachms of ointment, and the friction of a few days, will excite ptyalism, whilst those who are engaged for years, and during every variety of temperature, in preparing this article for sale, are never so affected? and yet in the history of one or two men, whose employment this was, and of whom I made it my business to enquire particularly, I could never find that they had been affected by mercury in the slightest degree.

Again, if mercury is not absorbed from the skin (as I hope to prove hereafter,) and if the absorbents which lie on the surface of the body are governed by the same laws with those that are seated in the lungs; what reason have we to believe that they will take up mercury, when those on the cuticle have refused to do so? should it though, be contended, that those two sets of lymphatics are governed by different laws; that those seated in the lungs are even as active as the lacteals; may it not be inferred, that if the last named set of vessels are so fastidious as to refuse to take up foreign matter, that the absorbents of the respiratory organs would also do the same? at least I can perceive no reason why we should deny the one set of vessels the power of absorbing extraneous matter, while we so liberally grant it to another.

If it be impossible then to explain the *modus operandi* of mercurial frictions, by calling to our aid the doctrine of cuticular absorption, the great question naturally occurs, how do they produce their effects?

On this subject, mysterious as it may appear, we are not I conceive more at a loss than we are to explain the mode of operation of any article of the *materia medica*, whether it be taken into the stomach, or applied to the skin. At one time, the absorption of medicines from either of those surfaces into the general circulation afforded an apt and satisfactory explanation of every phenomenon. Driven, though, by stubborn fact from this strong hold, we were compelled to resort to the doctrine of sympathy to explain, by a natural or acquired consent of parts, whatever that of absorption is inadequate to.

Thus, for example, by long habit, the actions of the stomach and salivary glands are so connected together by the principal of association, that when a substance is applied to the stomach calculated to produce a specific effect thereon, we invariably observe a correspondent effect to take place in the salivary glands. Mercury is a substance so calculated, and thus are the phenomena of salivation at once explained. Why not, then, extend this explanation to medicines applied to the skin? Why not suppose that habit or nature having established a certain consent between it and the salivary glands, mercurial frictions act in the same way?

Is it not in favour of this supposition, that the bark shirt cures intermittent fever? that tobacco nauseates, when externally applied? and that blisters excite strangury? when, though, in addition to this, we find that mercury applied to one side of the body has been known to affect that side only, this explanation will become still more plausible. Mr. Riche-rand, in his excellent treatise on Physiology, states, that a 'young man for whom' he had 'ordered frictions on the inner surface of the left leg and thigh, to resolve a large bubo, was seized with a salivation the third day, although only half a drachm of ointment had been used each night; the salivary glands on the left side only were swelled, the left half of the tongue was covered with aphthæ, the right side of the body remained perfectly free from mercurial influence:' an evident proof that ptyalism, in this instance, could not have proceeded from absorption, but must have been the result of some secret and unexplained sympathy. I am well aware, however, of the objection, that if the operation of frictions depended on sympathy, any other sialagogue ought to produce the same effects, when similarly applied. Thus strangury is often produced by the application of blisters, an effect hitherto ascribed to absorption; for it is contended, that if this effect resulted from sympathy, we ought to experience similar results from any of the siliquosæ, and yet no other visicating substance whatever excites this complaint.

In reply to this statement, it will be sufficient to observe that the objector has done little more than beg the question.

Pursue but for a moment this species of reasoning, and mark the consequences. If bark, for example, when applied to the surface of the body, cures intermittent fever by being absorbed, as it must necessarily do, according to this doctrine are we not authorised to expect equally beneficial effects from any article of the class of tonics, that could be as readily taken into the circulation as bark? hence, if this reasoning proves any thing, it is simply that cantharides is specifically different from any of the siliquosæ; that blisters will excite strangury, whilst cataplasms of mustard will not; for the same reason that a purgative does not generally prove an emetic, nor an emetic a purgative.

That no absorption of cantharides takes place in the case of strangury, I think sufficiently evident, for the following reasons?

First, If we admit of absorption in the above instance, we must also admit of specific determination to the bladder; and yet specific determination is more difficult of explanation than absorption.

Secondly, This effect ought not only invariably to occur, but, to take place in a short space of time, as the absorbents are supposed to be always in a state of activity.

Thirdly, If this symptom be referable to the principle of absorption, it ought to be proportioned, in its degrees of violence, to the size of the blister, and to its length of continuance, neither of which circumstances are noticed by authors on the subject.

Lastly, Because for strangury no remedy, according to the practice of some physicians, is better than the application of a second blister.

Here then is an effect produced from the skin on the bladder, in which the principle of absorption could have had no agency. How then can it be better explained, than by referring it to that great law of the animal economy called sympathy? It is vain to object that the doctrine of sympathy explains nothing; that it is vague and indeterminate in its signification, for when defined to mean no more than that consent of parts

which has invariably been remarked to take place under certain circumstances, it clearly furnishes as full and satisfactory an explanation of many of the phenomena of life, as our present imperfect knowledge of cause and effect will permit us to obtain, from any source whatever. Thus, for example, when I say that a dose of turpith mineral will speedily excite a flow of saliva, in consequence of a sympathy that subsists between the stomach and salivary glands, is not the explanation of antecedent and consequent as complete as when I say this effect depends on absorption.

It would be easy, in this way, to multiply arguments against the doctrine of absorption; but as my introduction is, perhaps, already swelled beyond its proper limits, and it is improper, on a subject so purely experimental as the present, to trust to analogy what can be made the subject of direct experiment, I shall proceed to relate such as I have made, in their proper order, and with the candour and impartiality of one who has no favourite theory to support, but whose only object is truth.

As the experiments of Dr. Rousseau, as far as I know to the contrary, are those that have excited most attention, or, at least, as they furnish almost all the direct evidence that we have against the existence of cutaneous absorption, I thought it my duty, for reasons previously assigned, to repeat some of the most important of them, and to communicate their results, before I proceeded to relate those which have been devised and executed by myself.

EXPERIMENT I.

With a view to determine whether spirits of turpentine would discover itself in my urine, by its characteristic property of imparting to it a violet smell, I took a few drops of it on a lump of sugar, and in the course of three hours, discovered a strong smell of violets in my urine.

EXPERIMENT II.

Several days after the above experiment, when I could not expect that a particle of turpentine remained in my system I proceeded to the following: At ten o'clock of a fine morning

in April, the temperature of my room being 65 degrees of Fahrenheit, and my pulse 60, its natural standard, after a light breakfast, I exposed myself to the emanations of spirits of turpentine in a closed room, by placing some of this fluid in a saucer, on a table, at which I was busily engaged in writing. In about an hour, I referred to my urine, and found the smell of violets as strong as in the foregoing experiment, where the turpentine had been taken in substance. There then remained no doubt but that the emanations from this fluid, entering the body, had been conveyed into the general circulation. The difficulty though, still remained: how had it entered the system? To ascertain this point, therefore, the following experiment was repeated.

EXPERIMENT III.

I provided myself with a long tin tube, by means of which I could, while sitting in my room, draw the air from a distant place without it, where none of the emanations, to which my body was exposed, could have previously existed, or have been conveyed during the experiment.

Thus provided, and in a room whose temperature was 75 degrees of Fahrenheit, I took the tube in my mouth, closed my nostrils, and immersed my hand and arm nearly up to the elbow in spirits of turpentine, when I directed the jar to be luted round my arm, in a manner that rendered it impossible that I should afterwards inhale any of the effluvia of this volatile substance.

Having previously taken a diuretic draught my assistant was enabled, at the end of an hour, to examine my urine, but could discover no smell of violets, the perspirable matter of my body, and my breath were also examined, and were found to have undergone no change. To render this experiment still more conclusive, another diuretic draught was taken, and it was determined to continue the immersion for an hour longer; but when half that period had elapsed, the pain became so intensely severe, that it could no longer be supported. The urine, perspiration, and breath were therefore again examined

but with the same results ; after which the hand and arm were withdrawn, swelled to nearly double their natural size, and so excessively painful, that I was unable to use my fingers or wrist for several hours. The pain and burning occasioned by this severe application gradually subsided, leaving behind them a scarlet redness, and great sensibility of the parts, which continuing for four or five days, terminated in a total destruction of the cuticle. From time to time, throughout the day, I repeated the examination of my urine, breath, and perspirable matter, but never could detect in them any thing that authorised the opinion that this substance was or could be absorbed from the external surface. Conclusive, therefore, as this experiment would appear, it has nevertheless been objected to it, that the surface exposed to the influence of the spirits of turpentine was too small, and that the activity of its absorbents might have been less than that of those seated on other parts of the body. To obviate this objection, I proceeded to

EXPERIMENT IV.

On a fine morning in April, the temperature of the room in which I proposed to make the experiment being between 70 and 80 of Fahrenheit, I adjusted my tube as formerly described, took off my clothes, placed the end of the tube in my mouth, stopped my nose, and directed my assistant to besmear my body and superior extremities by means of a sponge with spirits of turpentine. The pleasant sensations produced by the friction of the sponge counterbalancing the painful ones created by the turpentine, induced me to direct the attendant to continue it without intermission. The application of this fluid, therefore, so far from having been intermitted, was unceasingly continued throughout the experiment ; so that the absorbents had the additional advantage of friction, by which to drink up the substance with which their mouths were besmeared. Three quarters of an hour elapsed, when my assistant received my urine in a vessel as I sat, but could not discover the least smell of violets. The hour being completed my body was carefully washed. I then closed my mouth and

nose, and walking precipitately into the next room, examined my urine, without being able to detect in it the odour by which the absorption of turpentine was to have been proved. My friends also examined my breath, and were unanimous in saying that it had undergone no change. The common tests were frequently resorted to during the succeeding twenty-four hours, but with the same results.

EXPERIMENT V.

If, then, it is satisfactorily demonstrated by the foregoing experiments, that whenever spirits of turpentine manifests itself in the urine, breath, or perspirable matter, it could not have been introduced by means of cuticular absorption, it remains to prove by experiment what has already been assumed, that it enters the general circulation through the medium of the lungs. With this view, having introduced my long tube into the mouth of a large bottle which contained a little spirits of turpentine, I inhaled several successive times the emanations of this fluid. At the end of an hour, referring to my urine as a test, I found it strongly tinged with a violet smell. This experiment, therefore, while it proves the strong powers of absorption possessed by the lungs, amply accounts for the innumerable mistakes that have been made in ascribing the presence of certain substances in the general circulation to absorption from the skin.

Doctor Rousseau, after having related experiment No. 4, recommends it to those who may repeat it to be extremely cautious how they proceed, as the most trivial inattention might widely vary its results. To me, though, I hope this caution was unnecessary. I candidly confess, that the experiment was undertaken with the firm persuasion that conclusions would be drawn from it different from those that had been drawn; but having no favorite theory to support, I did not care to repeat it with the greatest circumspection. It was made in the presence of a worthy friend and ingenious fellow-graduate, who, believing that absorption from the external surface did actually take place, was so well convinced of its accuracy, that his opinions on this subject have thereby undergone considerable change.

Indeed, when I first heard the existence of this function questioned, I was thunderstruck with what I then believed to be the absurdity of the opposite opinion. I was in vain told, that a graduate in this university had experimented largely and with great acumen on the subject; that spirits of turpentine applied to the skin produced no sensible effect; that the poison of the viper, if brought into simple though close contact with the cuticle, was harmless; and that many of the phenomena hitherto ascribed to absorption had been distinctly traced to the inhalation by the lungs of the substances acknowledged to produce them. These were difficulties that my imagination readily surmounted. I admitted the facts, but gave very different explanations of them from those they had received at the hands of my opponents. The fundamental truth, that all medicines act specifically suggested itself as a weapon with which I could parry every objection, and confound my adversaries. Medicines, said I, when taken into the stomach, produce their several distinct and specific effects. The operations of opium are radically and invariably different from those of mercury, and the influence exerted on the animal economy, by a purgative no less different than that exerted by an emetic. Let us then extend this general law of the system. The *materia medica* abounds with some substances that have, and some that have not, a specific operation on the lacteals and internal lymphatics. Why not then generalize the principle, and say, that it also abounds with some that have and some that have not a specific operation on the external lymphatics. Spirits of turpentine and the poison of the viper, for example, have no known tendency to promote absorption on the part of the lacteals and internal lymphatics. But the absorbents, seated both on the surface of the body and internally, it may be fairly presumed, are governed by the same laws; surely, therefore, we have no right to expect that the above substances will stimulate to increased external absorption, when they have been found to produce no such effect on the self same congeries of vessels, as they exist in the different cavities of the body. But even though these two sets of vessels were admitted to be governed by different laws, still it would appear to follow, that the non-absorption of the spirits

of turpentine and the poison of the viper proved no more than that those substances have not that specific operation calculated to awaken the cutaneous lymphatics into action. Again too, it was further objected, that the highly acrid and stimulating substances, made use of in the experiments on this subject, were not such as were most likely to be taken into the general circulation, and of course that no reasoning from them could be received as conclusive.

To give, therefore, this question the certainty which it evidently deserved, it became necessary, not only to repeat the experiments previously related, but to extend them to a variety of articles, some of which at least should be milder and more bland than those heretofore employed. With this view the following series was instituted.

SECOND SERIES OF EXPERIMENTS.

EXPERIMENT VI.

To accomplish these important ends, I selected a substance which, from the uniformity with which it has been supposed to produce its effects through the medium of absorption, promised results highly satisfactory. The substance alluded to is mercurial ointment. To ascertain whether it did or did not produce salivation, together with its other effects, in consequence of being taken into the general circulation, I applied large plaisters of it to the calves of my legs. But as I was apprised of the objection, that the mercury might be volatilized and taken into the lungs, unless means were adopted to prevent it, and that if applied to an abraded cuticle, the experiment would be inconclusive, care was taken to avoid both the one and the other of these sources of error, by applying them to a perfectly sound cuticle, and by covering them with thick bladders, rollers, and a pair of stockings, which were not removed until the end of the experiment.

Thus circumstanced I waited patiently the arrival of a speedy ptyalism, but at the end of eight days was completely disappointed, never having observed the slightest alteration in my general health, nor in my pulse, nor in the discharge of saliva.

Not content though with this, and willing to believe that the result of my experiment had failed to correspond with my expectations only in consequence of want of attention to a restricted diet, I accordingly resolved to live exclusively on vegetables, and to eat even of these with moderation. This determination was immediately carried into execution, but with no better success; for, at the end of another week, the plaisters had certainly produced no sensible effect. Unwilling, though, to abandon a doctrine so generally acquiesced in, and ardently desiring to know something conclusive on this subject, I reduced my system still farther, by the loss of fifteen or twenty ounces of blood, continued my vegetable diet, and applied, with the former precautions, two large mercurial plaisters to my fore arms, where they were suffered to remain seven or eight days; at the end of which time, as they had produced no effect whatever, I put an end to the experiment, after its having lasted the greater part of three weeks.

EXPERIMENT VII.

From the foregoing facts it appeared, that I was fairly authorised to conclude that there was no active power of cutaneous absorption; since, if there was, I ought to have been salivated, as having lived under every circumstance essential to that event. Afraid, though, to trust this conclusion to conjecture, when it could be made the subject of direct experiment, and apprehending that my experiments might possibly have failed from want of activity on the part of the absorbents, to which the plaisters were applied, or from some idiosyncrasy of constitution, or from some imperfection in my mode of living, or in the quality of the ointment used, I resolved to try the effect of frictions. Accordingly, on the same parts, with much less of the same ointment than had ever been applied at any one time in the form of plaisters, I succeeded in three nights in gently affecting my mouth.

EXPERIMENT VIII.

From the foregoing experiment it would appear, that the absorbents, stimulated by the friction used in the application of the ointment, were compelled to take it into the circulation.

To determine, therefore, how far this was really the case, it was necessary that the ointment should be applied to parts which, at the same time that they possessed the advantages of friction, should leave nothing to apprehend from its being volatilized and taken into the lungs. Accordingly, I provided a strong pair of oil cloth socks, applied an ounce of unguentum hydrargyri fortius to the upper surface of each of my feet, put on the socks, drew a pair of stockings over them, and regularly walked a mile or two every day, that the friction of my boots against my feet might cause the mercury to be absorbed. At the end of ten days, though, I was greatly astonished to find that they had produced no effect. The socks were therefore taken off, and the same quantity of ointment again applied, after which they were renewed, and permitted to remain sixteen or eighteen days longer; but as in this time no alteration had taken place in the state of my salivary glands, they were again removed, and the experiment considered as concluded. It may not be improper to add, that my diet on this occasion was low, and strictly vegetable.

EXPERIMENT IX.

Apprehensive that there might not have been friction enough in the above experiment, to answer the purposes intended to be answered by this indispensable agent in the production of salivation by means of mercurial ointment, I was anxious to devise some remedy for this probable deficiency. For this purpose, it was recommended to apply strongly stimulating substances to my feet, which, at the same time that they excited the absorbents, would not impair the cuticle. With this view, having poured some boiling water on a quantity of bruised mustard seed, I immersed my feet in the

infusion, until the irritation became so great as to be almost unsupportable, when they were taken out, and the socks, with the same quantity of unguentum hydrargyri fortius used in the former experiment, were again applied. This done, I immediately set out on a long walk, that the mercury might be rubbed into my feet, and the stimulus of the bath be thus co-operated with by that of friction. The socks were worn a week ; but as at the end of that time no appearance of mercurial affection was to be discovered, they were taken off, and the experiment regarded as complete.

I am well aware, that this experiment may be conceived by some to militate against my explanation of the *modus operandi* of mercurial frictions ; since it may be asked, if they produce their effects in consequence of a sympathy between the skin and salivary glands, why this sympathy was not excited in my experiments with the mercurial socks ? The answer to this question is, I think, extremely obvious : for the feet are so far removed from the centre of circulation, and so far from the salivary glands, that it could not be expected they would be readily brought into sympathy with each other. Hence ptyalism was not produced : and for the analogical reason that ‘ children under a certain age cannot be salivated, because those two sets of vessels’ (the stomach and salivary glands) ‘ have not acted long enough together for their motions to become associated. *’

EXPERIMENT X.

As I had now made every experiment with mercurial ointment that promised any thing conclusive on this subject, I thought it not improper to direct my attention to such other substances as should, by certain characteristic properties, enable us to detect them in the general circulation, should they be absorbed. Being therefore provided with large cataplasms of bruised garlic, I disposed of my tube as formerly described, took the end of it in my mouth, closed my nose, and directed my assistant to apply the plaister under the exilla of both arms.

* See Dr. Young’s Inaugural Dissertation.

In the selection of this place, as the most proper for the application of the garlic, I was influenced principally by two reasons ; 1st. Because it appeared to me, that if garlic failed to manifest itself in the urine, when applied to the feet, it might have proceeded from the languor of circulation in those parts ; and 2dly, Because having been informed that mercurial applications succeeded no where so well as under the arms, it appeared clearly the most eligible disposition of my cataplasms that could be made. Thus situated, I suffered them to remain an hour, at the expiration of which time they were removed, and the parts carefully washed, when I quitted the room, with every precaution to avoid inhaling their penetrating odour. My urine, breath, and perspirable matter were now examined, but did not, at this or any subsequent period, discover the least smell of garlic.

EXPERIMENT XI.

Having heard the following fact advanced in favour of the absorption of foreign matters into the general circulation, I determined to repeat the experiment.

It is said, if a strong ligature be made round the arm, so as to stop the circulation in the subcutaneous veins, and that if the corresponding hand be then immersed in a strong solution of nitre, while it is occasionally chafed by the other hand, that the nitre will be absorbed. To prove which it is affirmed, that if blood be drawn from a vein in which its circulation had been stopped, and then dried on a piece of paper, it will flash when burned, as if containing nitre. Suffice it to say, that I repeated this experiment, with the precaution of washing my hand before the blood was drawn, and that I found the assertion to be entirely unfounded.

Thus from a careful investigation of facts, have I been obliged to relinquish my former opinions, and to acknowledge that the function of cuticular absorption has no other claim to our belief than the sanction of hoary-headed authority, than mental apathy, or inattention to the evidences by which it has been supported.

In thus denying the existence of cutaneous absorption, far be it from my intention to fly so directly in the face of anatomical demonstration as to deny that there exists a set of absorbents beneath the skin. All I contend for is, that they have not the power of conveying foreign substances into the general circulation, whether these substances be applied to a sound or abraided cuticle. To prove that such is not the case, in the first instance, nothing more, I presume need be said; and that it is not in the second, may, I think, be fairly inferred, from the following statements:

First, As the lacteals will not carry extraneous matter into the general circulation, so we have no reason to believe that the lymphatics will.

Secondly, Because the spirit of turpentine was not absorbed in two of my experiments in which the cuticle was destroyed.

Thirdly, From the analogical fact, that mercurial frictions act by sympathy, and not by absorption*.

Fourthly, Because congestions and obstructions have never been detected in the course of the lymphatics.

And lastly, Because it is easy to find a much wiser and better employment for this system of vessels, than that of converting our blood into one indescribable mass of heterogeneous matter. The arteries carry to the various parts of our bodies the matter out of which they are formed; the lymphatics mould and give to it its proper shape. One part is redundant, they prune it to its proper standard; another deficient, the arteries furnish the proper materials for its enlargement, the lymphatics manufacture them.

It is now time that I should put a period to this essay. How far I have succeeded in establishing the points contended for, is not for me to determine. All that I can ask, or flatter myself with obtaining, is, that the good sense of every person who dispassionately considers the subject will prevent the attempt from being considered as chimerical.

* This argument is conclusive, for the cuticle is often destroyed, where the frictions have been continued for a long time, or have been very violent.

AN INAUGURAL DISSERTATION,
UPON THE
THREE FOLLOWING SUBJECTS,

- I. AN ATTEMPT TO PROVE, THAT THE LUES VENEREA, WAS NOT INTRODUCED INTO EUROPE FROM AMERICA.
- II. AN EXPERIMENTAL INQUIRY INTO THE MODUS OPERANDI OF MERCURY, IN CURING THE LUES VENEREA.
- III. EXPERIMENTAL PROOFS THAT THE LUES VENEREA, AND GONORRHOEA, ARE TWO DISTINCT FORMS OF DISEASE.

SUBMITTED TO THE EXAMINATION OF THE

REVEREND JOHN EWING, S. S. T. P. PROVOST,

THE TRUSTEES AND MEDICAL PROFESSORS OF THE UNIVERSITY
OF PENNSYLVANIA,

ON THE EIGHTH DAY OF JUNE, A. D. ONE THOUSAND EIGHT
HUNDRED AND ONE.

FOR THE DEGREE OF DOCTOR OF MEDICINE,

BY JAMES TONGUE, OF MARYLAND:

HONORARY MEMBER OF THE PHILADELPHIA MEDICAL AND
CHEMICAL SOCIETIES.

‘No man ought to surrender his own judgment to any mere
authority, however respectable.’

Priestley on Phlogiston.

INTRODUCTION.

IT will be thought presumptuous that the author, a mere Tyro in the science of medicine should attempt to write upon three subjects, which have, for centuries, agitated the medical world.

If there be no information afforded in the following pages he will, at least here, be on a footing with many of his superiors.

It would be impossible, in the compass of a discourse like the present, to enter minutely into each of the different subjects embraced in this dissertation. It would require years instead of the few weeks which are allowed to the candidate, to write a thesis; and a mind pregnant with information, instead of one which can only be in its infancy.

The first part of this dissertation; must necessarily consist in a collection of such facts, as he has been able to procure.

Most people, have such an aversion from being experimented upon, particularly with the venereal virus, that it has prevented the second part of this dissertation from being treated in such an ample as could be wished.

I cannot forbear mentioning here, that the experiments contained in the following pages, were made with the greatest care, and related with the strictest fidelity; yet I am sensible, many inaccuracies may have escaped; which those will most readily excuse, who have experienced the difficulties incident to such researches.

INAUGURAL DISSERTATION.

AN ATTEMPT TO PROVE, THAT THE LUES VENEREA WAS NOT
INTRODUCED INTO EUROPE FROM AMERICA.

The origin of the disease, now called lues venerea, has been a subject of much debate among most medical writers. The greatest part of these inform us, that it was brought by Columbus and his companions from the West Indies, between the years 1494, and 1496, *merely* because it happened to rage with great violence in Europe, shortly after the return of those navigators from their first voyage—like the yellow fever no person willing to give it birth, fruitlessly seek for its origin in ships and sailors, that have arrived from foreign countries.

The proofs of those, who have given this origin to the venereal disease, are all equivocal, and much greater counter demonstrations must be adduced, before the scale will preponderate on that side of the question; though now it appears to be the most prevalent opinion, as scarcely a book on this subject can be opened, but we are informed that this disease was introduced into Europe from America.

To find out the precise time of its first appearance, would perhaps be as fruitless as the search after the philosopher's stone. The limits of this dissertation will not admit me to enter in so minute an investigation of its origin, as could be wished.

We will offer here a few remarks upon one of the oldest books we have in existence—namely, the Bible.*

*No person will think there is the least impropriety in making the following quotations from the Bible. The same causes existed then as exist now for the production of this disease; and in those times other diseases existed which were a thousand times worse.

It has been said by some authors, that 'the sore boils' wherein Satan smote Job by God's permission, 'from the sole of his foot to the crown of his head,' was the venereal disease.

In ch. 20. v. 11. he says,—'his bones are full of the sin of his youth, which shall lie down with him in the dust.'

It has been said that the following verses lead us to believe that David had the same disease. Psalm 38. v. 3. 'There is no soundness in my flesh, because of thine anger, neither is there any rest in my bones, because of my sin.'

V. 5. 'My sores are putrified and corrupted, because of my foolishness.'

V. 7. 'For my loins are filled with a loathsome disease, and there is no soundness in my flesh.'

It is said in Ecclesiasticus, ch. 19. v. 3. as follows,—'And he who joineth himself to harlots, will be naught, rottenness and worms shall inherit him.'

The following verses, we find in the 15th chapter of the book entitled Leviticus, commonly said to be written by Moses. They are part of a law given by him in order to prevent a disease from spreading, which appears to have been a true gonorrhœa.

'Leviticus ch. 15. v. 2. 'The man that hath an *issue of seed* shall be unclean.'

V. 3. 'And then shall he be subject to this evil, when a filthy humour, at every moment, cleaveth to his flesh, and gathereth there.'

V. 4. 'Every bed, on which he sleepeth, shall be unclean, and every place on which he sitteth.'

V. 5. 'If any man touch his bed he shall wash his cloathes; and being washed with water, he shall be unclean until the evening.'

V. 6. 'If a man sit where the man had sitten, he also shall wash his cloathes; and being washed with water shall be unclean until the evening.'

V. 7. 'He that toucheth his flesh, shall wash his cloathes; and being himself washed in water shall be unclean until the evening.'

V. 8. 'If such a man cast his spittle upon him that is clean, he shall wash his cloathes; and being washed with water, he shall be unclean until the evening.'

V. 9. 'The saddle on which he sitteth shall be unclean.'

V. 10. 'And whatsoever has been under him, that has the *issue of seed*, shall be unclean until the evening. He that carrieth any of these things shall wash his cloathes; and being washed with water he shall be unclean until the evening.'

V. 11. 'Every person whom such a one shall touch, not having washed his hands before, shall wash his cloathes; and being washed in water, he shall be unclean until the evening.'

V. 12. 'If he touch a vessel of earth it shall be broken; but if a vessel of wood, it shall be washed with water.'

V. 13. 'If he who suffereth this *disease* be healed, he shall number seven days after his cleansing; and having washed his cloathes, and all his body in living water, he shall be clean.'

V. 16. 'The man from whom the *seed of copulation* goeth out, shall wash all his body with water; and he shall be unclean until the evening.'

V. 17. 'The garment or the skin that he weareth shall be washed with water; and he shall be unclean until the evening.'

V. 32. 'This is the law of him that hath the *issue of seed*, and that is defiled by *copulation*.'

We see that the disease of the Jews, called the *issue of seed*, as described in the preceding verses, was contagious to the greatest degree, and also propagated by *copulation*.

To prove that this was a gonorrhoea, it is only necessary to observe that this disease has till very lately been supposed to consist in a flow of semen*. Hence the word gonorrhoea is derived from the Greek words *γενε* *genitura* *semen* and *ρῶ*

* Astruc, a late writer defines this disease to be a discharge of semen from the urethra.—See his description of it, vol. 1.

fluor, i. e. fluxus seminis. And upon looking into this part of the Greek bible we find the word *γενεσις* i. e. *seminis genitalis profluvium*, which is translated in the bible *an issue of seed*.

It is said by one of the prophets, ‘fly the person afflicted with the *Judham* as you would a lion.’

Judham being an Arabian word which signifies an inveterate degree of the venereal disease, caused by improper treatment*.

The inhabitants of Indostan have, time immemorial, made use of the word *Khorah* to signify the same thing†.

The venereal disease has been known to the inhabitants of Indostan *time immemorial*, by the name of the *Persian fire*.

This disease has been attributed by them time out of mind to the Persians, as the Europeans have attributed it to the Americans, the French to the Neapolitans, the English and Germans to the French, &c. This disease certainly existed among the Greeks and Romans.

The disease to which the Scythians were subject according to *Herodotus*, and also according to *Hippocrates*,‡ who have both named it *morbis femininus* (*Swediaur* says) seems to have been a true gonorrhoea||. The last of these authors, (*Hippocrates*) speaks of defluxions from the private parts, exulcerations, tubercles in the groins, large spreading ulcers, abscesses and suppurations, mutilations of the bones and nerves§.

In his book ‘*de morbis mulierum*’ he makes mention of the suppuration of the inguinal glands, and he attributes it to the suppression of the menses, which we now know, will not cause a suppuration of those parts.

* See Asiatic Researches, vol. 2, and *Swediaur’s maladies syphilitiques* dernière édition à Paris, 1798. Tom. 1.

† Ibid.

‡ Hippocrates wrote 460 and Herodotus 404 years before the Christian era.

|| Voyez *Swediaur*, dernière édition, à Paris, 1798.

§ He speaks of these in his epidemic diseases.

Juvenal, in Satyre XI, who wrote in 128, and *Martial*, lib. viii. and ix. who wrote in 100, speak of ulcers in the genital parts, as diseases produced by impure coition.

Plutarch, § in his life of Otho, about the middle of the first century, speaking of Tigellinus, says that he was afflicted with several diseases, which he had contracted by conversing with lewd women.

In *Pliny* the second, lib. 6, Epis. 34, (who wrote in 97) we find a disease of the falling off of the genital parts, which was regarded as incurable, ‘*Maritus ex diutino morbo circa velenda corporis ulceribus putrescebat.*’

Diascorides, (who wrote in 50) recommends remedies against rhagades, condylomata, maligna ulcera, vulvæ tubercula, genitalium et vulvæ exulcerationes.*

Sextus Placitus, papyriensis parabil. medicament. script. antiq. speaks of remedies against ‘*bubones seu tumores ad inguina, carbunculos in veretro, ficos in ano, rhagades, phymata, callos in veretro.*’

Galenus, (*opera per 1. Cornar.*) who wrote in 200, speaks of phymosis, paraphymosis, rhagades, condylomata, bubones, phymata purulenta, acrochordones, &c.

Bishop Palladius, who lived in the fifth century, under the reign of Theodosius, junr. relates the following curious anecdote of a hermit named *Heron*, who had always led a very virtuous life.

From the Greek of *Palladius*, it has been translated into French, from which I have translated it into English.

‘At last, *Heron*, seized by the influence of evil genius, and transported as if by a devouring fire, could not remain confined to his cell. He sets out all at once for Alexandria: the design of God called him there, and according to the proverb, drove a nail by the other*.’

‘Indeed he fell into the forgetfulness of his duties, that were to lead him, in spite of him, to his safety. He frequented theatres, horse races, and spent his life in taverns. From

§ Vol. 8. p. 303.

* That is to say, pride, by the humiliation of his fall.

the excess of good cheer and wine, he fell into the abuse of women, and the most dirty libertinage. Having resolved to sin, he had a habitual commerce with a dancer of pantomimes, and declared to her the evil, (or wound) that tormented him. In the mean while there came upon the gland of his genital parts, a carbuncle or anthrax (probably what we would call a chancre). The disease grew so bad in the space of six months, that his genital parts were mortified and fell off.' This is a sufficiency of the anecdote for our purpose, and every body will think with me that it was a case of confirmed syphilis.

Celsus, (who wrote in the first century) in his book 4. ch. 31, speaks of 'nimia profusio seminis sine venere, et sine nocturnis imaginibus. That is to say, a true gonorrhoea, which became at last fatal, in causing by degrees a consumption. And in book 6, ch. 18, he speaks of ulcers in the genital parts.

Guillelmus de Saliceto, placentinus chirurg. lib. 1, ch. 42. (who wrote 223 years before the discovery of America) speaks 'de apostemate in inguinibus. Hæc ægritudo vocatur bubo vel dragoncelli inguinis.' And he says in the same sentence, 'et aliquando accidit homini in virga corruptio propter concubitum cum fœdâ muliere, aut ab aliam causam.' Again, in ch. 48, he speaks, 'de pustulis albis vel rubris, et de milio et de corruptionibus circa preputium, propter coitum cum fœda muliere, aut cum meretricè, aut ab aliâ causâ.

Guido de Cauliaco Gabalanus, doctor of physic and surgeon in the university of Montpellier, chirurg. mag. (who wrote 133 years before the discovery of America) speaks 'de califactione et fetiditate in virgâ propter decubitum cum muliere fœdâ.'

Valescus de Tarenta, professor of Montpellier, lib. 6. (who wrote 93 years before the discovery of America, speaks. 'De ulceribus et pustulis virgæ.' Again he says, 'pustulæ virgæ fiunt si quis coeat cum famina habente ulcus in matrice, quæ contagio, sitate sua inficet virgam et in ea facit ulcus.'

Petrus de Argelata of Bologna, Doctor in arts and physic, in his chirurg. lib. 2. who wrote 23 years before the discovery of America, speaks; 'pustulis quæ adveniunt virgæ prop-

ter conversationem cum fœdâ muliere, quæ albæ sunt vel rubræ. Ex materiâ venenosa (says he) quæ retinetur inter præputium et pellem virgæ causantur istæ pustulæ, &c.

Lanfranc of Milan, Doctor of Physic who wrote in 1290. *Benedictus Gordonius*, who wrote in 1300, professor of physic at Montpellier; and *John* of Gaddesden an English surgeon who wrote in 1301, all speak of the same affections of the genital parts as the preceding writers.

The following is taken from a M. S. in Lincoln College in Oxford * (written 63 years before the discovery of America.)

‘Novi enim ego Thomas Gascoigne, † licet indignus, sacræ Theologiæ Doctor, qui hac scripsi et collegi, diversos viros qui mortui fuerunt ex putrefactione membrorum suorum genitalium et corporis sui; quæ corruptio et putrefactio, ut ipsi dixerunt, causata fuit per exercitium copulæ carnalis cum mulieribus. Magnus enim dux in Anglia, scil. I. de Gaunt, ‡ mortuus est ex tali putrefactione membrorum genitalium et corporis sui, causatâ per frequentationem mulierum, magnus enim fornicator fuit, ut in toto regno Angliæ divulgabatur, et ante mortem suam, jacens sic infirmus in lecto idem eandem putrefactionem regi Angliæ Ricardo secundo ostendit, cum idem eundem ducem in suâ infirmitate visitavit; et dixit mihi qui ista novit unus fidelis sacræ Theologiæ Baccalaureus. Willus etiam longe vir maturæ ætatis et civitate Londoni, mortuus est ex tali putrefactione membrorum suorum genitalium et corporis sui, causatâ per copulam carnalem cum mulieribus, ut ipse met plures confessus est anti mortem suam, cum manu suâ propriâ eleemosynas distribuit, ut ego novi, Anno Domini 1430.

It is evident from what has been said, that there existed, long before the discovery of America, diseases of the genitals, similar from their description to the venereal disease, which

* See Beckett on the antiquity of the venereal disease, in the philosophical transactions abridged by Eames and Martin, vol. 7.

† Chancellor of Oxford.

‡ John, Duke of Lancaster, called by the English, John de Guant.

was communicated in the same way that this disease is, and which certainly was the same disease.

The following quotation, is taken from the statutes for the regulation of the public stews, established at Avignon in 1347, by Jane 1st, Queen of both the Sicilies and Countess of Provence, written 145 years before the discovery of America.

ARTICLE 4*.

‘ The queen commands, that on every Saturday the women in the house singly be examined by the abbeſs and a ſurgeon appointed for that purpoſe by the directors, and if any of ’em has contracted any illneſs by their whoreing, that they be ſeparated from the reſt, and not ſuffered to proſtitute themſelves, for fear the youth, who conſerve with ’em, ſhould catch their diſtempers.’

In the chronicle of Cardamus we read that Ladislas king of Naples died in the year 1414, in conſequence of an infection in the genital parts communicated by his miſtreſs†.

It is ſaid that Alphoſus, likewiſe king of Naples, died in 1458, of an inveterate gonorrhoea; theſe words are related of him—‘ anxius deceſſit, morbo inſuper immundo et pertinaci, involuntario ſcilicet inſenſibilique *spermatis fluxu*†.’

The venereal diſeaſe appears to have exiſted in England, many centuries before the diſcovery of America, under the name of brenning or burning.

Mr. Beckett|| informs us that ſtews were allowed to be kept on the bank ſide in Southwark under the juriſdiction of the biſhop of Wincheſter. He ſays there were anciently 18 of thoſe ſtews, but in the reign of Henry VII. they were reduced to twelve.

He has examined the records that relate to the regulation of theſe ſtews—The following are ſome extracts from them.

‘ We find that as early as the year 1162, divers conſtitutions

* Vide Aſtruc upon the Venereal, vol. 1.

† See Sanchez.

‡ See Sanchez.

|| See Philoſophical Transactions abridged by Jones, vol. 5, p. 381.

relating to the lordship of Winchester (being confined by the king) were to be kept for *ever* according to the old custom that had been *time out of mind*.*

After mentioning several rules we find the following—
'No stewholder to keep any woman that hath the perilous infirmity of burning.'

It appears that this brenning or burning had existed at a much more early period, as these regulations were only a renewal of such as had been before established time out of mind.

But to confirm this further—I find, (says Mr. Beckett that in the custody of the Bishop of Winchester, whose palace was situated on the bank-side near the stews, was a book written upon vellum, the title of which runs thus, 'Here beginne the ordinances, rules and customs, as well for the salvation of mannes lif, as for to aschewe many mischiefs and inconvenients that dayly be like there for to fall owte, to be rightfully kept, and due precaution of them to be done unto any personne within the same.'

One of the articles begins thus 'De his qui custodiunt mulieres habentes nefandam infirmitatem.' It goes on then 'That no stew-holder keep noo woman within his hous that hath any sycknesse of brenning, but that she be put owte unto the peyne of make it a fyne unto the Lord of a hundred sheleyllings.'

Mr. Beckett informs us this was taken from the original manuscript which was preserved in the bishop's court, supposed to be written about the year 1430.

John Arden Esqr. * who was one of the surgeons to king Richard II, and also to king Henry the IV, defines this brenning or burning to be a certain inward heat and excoriation of the urethra, which definition is sufficient to inform us that it was a true gonorrhoea.

The same author (John Arden Esqr.) who wrote about 114 years before the discovery of America, speaking of the cure for this burning, has the following words. 'Contra incendium. Item contra incendium virgæ virilis interius ex

* See philo. trans. abrid. vol. 5, p. 583.

calore et excoriatione, fiat talis syringia, (i. e. injectio) lenitiva. Accipe lac mulieris mascularem nutrientis, et parum zuccarum, oleum violæ et ptisanæ, quibus commixtis per syringam infundatur, et si predictis admiscueris lac amigdalorum melior erit medicina. †

Mr. Beckett ‡ informs us that in an old manuscript he has in his possession, written about 1390, is a receipt for brenning of the pyntyl yat men clepe ye, ape galee. Galee being an old english word for a running sore.

And in another manuscript written about 50 years after, is a receipt by a woman.

It is certain that the disease, from the description of its symptoms as given by the ancients, was often in an inveterate state, for which they appropriated particular names, perhaps more significant and expressive than those imposed by modern authors. Thus, for instance, buboes in the groin they called dorsers, from their protuberancy or lurching out. And the venereal nodes on their shin bones, they called boon or bone hawe; which gives us not only a just idea of the part diseased, but in what manner it was affected; Hawe being an old english word for a swelling.*

We have seen the word brenning or burning mentioned as early as the year 1162, and used at different periods up to the time of the discovery of America; we will now proceed to prove it has been used by many writers since that time, to signify the venereal disease.

Andrew Boord, a Doctor of Physic and Romish priest, in his book entitled the breviary of health, printed in 1546, speaks very particularly of this sort of burning. One of his chapters begin as follows: 'The 19th chapter doth shew of the burning of an harlotte,' and he says, 'that if a man be burnt with an harlotte, and do meddle with another woman within a day, he shall burn the woman that he shall meddle withal.' In his 82nd chapter, he speaks of two sorts of burning the one by fire, and the other by a woman through carnal

* See Philo. Trans. abrid. vol. 7.

‡ Ibid.

* Ibid.

copulation and refers the person that is burnt of an harlotte, to another chapter of his advice what to do, 'if he get a dorser or two.'

Mr. Becket * says in a manuscript I have, of the vocation of John Ball to the bishopric of Ossory in Irèland, written by himself, he speaks of *Doctor Hugh Weston*, (who was dean of Windsor in 1556, but deprived by Cardinal Pool for adultery as follows, 'At this day is lecherous *Weston*, who is more practised in the art of breech-burning than all the whores of the stews.' Again, speaking of the same person, he says, 'He not long ago brent a beggar in St. Botolph's parish.' The same author, says of him elsewhere; 'He has been sore bitten with a Winchester goose, and was not yet healed thereof; which was a common phrase for the pox at that time, because the stews were under the jurisdiction of the Bishop of Winchester. And we see, that they have been under his jurisdiction as early as the year 1162.

Simon Fish, a zealous promoter of the reformation, in the reign of Henry the VII. in his supplication of beggars, presented to the king in 1530, says as follows; 'these be they (speaking of the Romish priests) that corrupt the whole generation of mankind in one realm, that catch the pocks of one woman and bear it to another; that be *burnt* with one woman and bear it to another.'

William Bullien a physician in the reign of queen Elizabeth, in a book published, called the Bulwark of Defence, printed in 1553, brings in sickness, demanding of health what he would do with a disease called the French pockes, health answers, 'he would not that any should fish for this disease; or to be bold when is bitten, to think thereby to be helped, but rather to eschewe the cause of this infirmity, and filthy rotten burning of harlots.'

What has been related, sufficiently proves that the venereal disease existed in England, many centuries before the discovery of America, under the name of burning or brenning.

Delfilini speaks of having seen a man infected by the venereal complaint in 1491†.

* See Philoso. Trans.

† See Sanchez.

Pintor, before he went to Rome, met with an old man aged sixty, in the same condition, at Valencia, in Spain. This happened before the year 1493.

J. Zacharius Platner, in his opuscula, says, ‘hoc extra dubium videtur, longè antiquam India occidua inventa est, ægritudines fuisse observatas, quibus nunc venerei opprimuntur†.

In a letter written by *Petrus Martyr*, in April 1488, we find the following words, *Petri Martyris Anglerii mediolanensis* epist. 68. Ario Lusitano, Græcas litteras salmanticæ propositenti, valetudinario.‡

In peculiarem te nostræ tempestatis morbum, qui appellatione Hispana Bubarum dicitur (ab Italis morbus Gallicus, medicorum Elephantiam alii, alii aliter appellant) incidisse præcipitem, libero ad me scribis pede. Lugubri autem elego calamitatem, ærumnasque gemis tuas, articulorum impedimentum, internodiorum hebitudinem, juncturarum, omnium dolores intensos esse proclamas, ulcerum et oris fæditatem superaddit in promis eloquentia, conquereris, lamentaris deploras, &c*.

If there be no mistake in the date of this letter (which I dare say there is not) it is sufficient to make every person believe that this disease was not carried from the West-Indies into Europe.

Helius Capreoli de rebus Brixianorum, lib. 12, written in 1492, (before Columbus arrived in Europe from America) after having in common with *Pintor* and *Delphini*, recited the calamities which they had just experienced, and which still continued to exist, expresses himself in these words—‘Ab

† See Sanchez.

‡ Voyez Swediaur. Tom. 2, Introduction, p. 16.

* Sanchez takes notice of this letter, in his treatise upon the origin of the venereal disease, and it is dated in his book April 5th, 1489, he thinks is most probable it was dated some years after, but he is not certain, as Swediaur has written a good many years since the publication of Sanchez’s treatise, and must have seen it; we may conclude, that he would not have quoted this letter, without being certain of its date.

inguine mulieribus, à glande viris sæpius inceptit; mox per universum corpus vagabatur: sensère id malum præsertim incontinentes, contactu tamen inficiebat quoque vicinos, audivimus omnem ferè terræ orbem invasisse genus id contagionis *morbum gallicum*, * nuncupatum quem hæc tenus, ut elephantiasin ante Pompeium magnum et ante Tiberium Claudium mentagram, Italia nunquam creditur esse passa.'

From the preceding pages it is evident, that the genital parts of both sexes, at all times, and perhaps in almost all countries, have been subject to the same diseases as those produced at present by the venereal or syphilitic virus.

The gonorrhoea of the Jews, the ulcers of the genital parts, the mortification of the penis, the swelling and suppuration of the inguinal glands, the different excoriations, the rhagades, &c. all described by the ancient Greek and Latin authors. Its existence in Indostan time immemorial by the name of *persian fire*. The carbuncles and falling off of the penis of Heron in the fifth century at Alexandria. The different venereal affections of the genital parts produced 'propter decubitus cum muliere foedâ'—the cases related by the chancellor of Oxford concerning I. de Gaunt and Wills; the different regulation of the public stews, &c. are satisfactory proofs of the antiquity of this disease. We will now proceed to say a few words more, in order to prove that this disease was not introduced into Europe from America.

Goncalo d'Oviedo, was the first and only author in those times, (about the end of the fifteenth century) who asserted that the venereal disease was brought from the island of St. Domingo, during the second voyage of Columbus, at his return, on the 8th of June, 1496. In his first voyage, d'Oviedo has not spoken of the venereal disease †. D'Oviedo was not a physician, and his authority, of consequence, is of very little importance in deciding this question. We are induced to believe that his ascribing this origin to the venereal disease was altogether conjectural from the following sentence.

* It is notorious that venereal disease wept by the name of *Morbus Gallicus* for centuries after the discovery of America.

† See Sanchez.

D'Oviedo, qui, au rapport de Fallope, s'étoit infecté à Naples, fut assez ingénieux pour conjecturer que son mal venant des Indes occidentales, il trouveroit aussi aux Indes le plus puissant spécifique ou la meilleure recette.*

It seems by this that the proofs, if they may be called so, of d'Oviedo are mere conjectures. It is said that he discovered the guaiacum by which he was cured.

M. de Paw,† speaking of the origin of this disease has the following words. ‘Ce qui prouve, sans réplique, que la peste Vénérienne est née en Amérique, c'est la quantité de remèdes auxquels les peuples de ces contrées avoient eu recours pour en retarder les progrès extrêmes: ils us oient de plus de soixant simples différentes, que le danger pressant les avoit forcés à connoître.’

Upon such trifling and frivolous assertions do they attempt to prove that this disease was introduced into Europe from America. If we may be allowed to reason in the same way as M. de Paw has done this disease ought to have originated in that country where there exists its only antidote, namely mercury.

The venereal disease certainly did not exist among the aborigenes of America.

Professor Barton, has informed me, whose information on this subject is superior to any writer, that the Indians constantly say, that the venereal disease was introduced among them by the whites, and that they cannot cure the disease; but when afflicted with it, always, if possible, have recourse to the whites, in order to be cured.

Petrus Pintor, who wrote a book in 1500, entitled ‘*De Morbo Foedo et Ocultis his Temporibus Affligentii &c.*’ says this (meaning syphilis) originated in all the world at the same time, and ascribes its origin to the influence of the stars, &c. Is it possible that if the disease had been brought from the West Indies, he would not have known it, and ascribed it to that source rather than to the influence of the stars? Even allowing him to be superstitious, would he not have taken notice of opinions on this subject contrary to his own? †

* Voyez Recherches Philosophiques, sur les Americains, par M. de Paw, tome premier, p. 17.

† Ibid.

Pintor, in the fourth chapter of his work, entitled '*Aggregator Sententiarum*,' shows that the venereal disease existed in Italy eight days after the return of Columbus, into Spain, from his first American voyage. He makes the following observation. 'Talis autem epidemia in urbe romana contigit, Anno 1493, mense martii, post introitum solis in primum minutum Arietis.'

Now the first return into Spain of Columbus, from St. Domingo, was on the 13th of March, 1493.

Is it possible, that the venereal disease, in the space of eight days, should have been communicated from the coast of Spain, to Rome?

If this disease had been brought from America, would such a variety of causes in those times, (about the end of the fifteenth century) been assigned for its origin? — 'As the fatal influence of the stars.' † — 'The baneful conjunction of the planets as Jupiter, Mercury, &c.' — 'The indisposition of the air.' — 'From eating human flesh.' — 'Lying with diseased beasts;' and a variety of others too tedious to mention.

Each of those opinions, of its origin, is given by different authors and written shortly after the discovery of America—had there indeed been any possibility of this disease having been brought from the West-Indies, would all those authors have ascribed it to such visionary causes as they have.

Astruc has taken much pains to prove that this disease was introduced into Europe from St. Domingo. He says ‡ that Columbus arrived in Cadiz from St. Domingo in 1496, with two ships, on board of which there were two hundred soldiers infected with the *venereal disease*..

D'Oviedo, who has ascribed the origin of this disease to the same source as Astruc, would certainly have availed himself of this fact if it were the case. He (Oviedo) has taken notice of the diseased state of Columbus' soldiers, and mentions that the 'Indians*' in consequence of the ill treatment they received from the Spaniards. would not sow the Indian corn, which was their only nourishment. A dread-

† See Astruc, on the venereal, vol. 1.

‡ See his book on the venereal, vol. 1. ch. 9.

* See Sanchez.

ful famine was the consequence of this obstinancy. The Spaniards to prevent their perishing through hunger, were obliged to eat insects of every description; and these depraved aliments together with the extreme humidity of the climate, induced a general sickness. In this state, Columbus left St. Domingo, on the 10th of March, 1496, on his return to Spain, having on board the two ships under his command two hundred and twenty-five sick soldiers. D'Oviedo does not say a word of the venereal disease in this place; he confines himself simply to the above sickness, as the following extract will shew—‘The half of the crew perished, at sea through hunger. The island was covered with the bodies of dead Indians, the stench of which was so great and pestilential, as to disease both natives and Spaniards.’*

It appears certain that the 200 soldiers, who Astruc said were infected with the venereal disease, must have been diseased from famine, and the pestilence, as mentioned above.

If this disease had existed among the inhabitants of America, would it not have been mentioned by some of the numerous historians who have written upon this country; which we do not find to be the case.

The greatest proof, (if I may so call it,) that they have in ascribing the origin of this disease to America is, that the guaiacum is a native of the west-Indies. This every body will allow is no proof at all.

The facts which we have related in the preceeding pages, to prove that the venereal disease was not introduced into Europe from America, are so positive, that the most ignorant sceptic cannot refuse his assent.

It is really strange to see the inconsistency of the authors who have written upon this subject, nearly all of them say that the venereal disease was first discovered among the Indians; and *still* it is said, that this disease was inflicted upon us by the Supreme Being, as a punishment for the unlawful gratification of our venereal appetites. It is universally acknowledged that the venereal propensities among the Indians are in a very feeble state.

* See Sanchez.

Now, is it presumable, that the Supreme Being, would inflict a disease upon a poor virtuous set of people, for the unlawful gratification of an appetite which they scarcely possess?

Error is never consistent with itself.

Before entering upon the *modus operandi* of mercury in the cure of the venereal disease: it will be necessary to make a few observations upon the venereal virus.

In those substances, where it is impossible to know their nature by analysis or separation of their component parts, the only means we have to pursue, is to study scrupulously their effects, to compare them with those with which we are best acquainted, to remark all their analogies, and to draw all the possible conclusions.

We have no correct knowledge of the venereal poison. We only know its effects, and that it is a morbid secretion.

It is generally said that this disease was inflicted upon us by the Supreme Being, as a punishment for the unlawful indulgence of our venereal appetite. This is as foreign from truth, as it is unjust to our Maker. God has nothing to do with inflicting diseases upon us. (With much reverence would I speak it.) They are all the production of our carelessness.

Does God cause a yellow fever, or is it caused by our negligence, in not timely removing the putrifying substances from our streets? Does God cause a pleurisy or consumption, or is it the effect of not accommodating our dress to the different changes in the atmosphere?

I would rather suppose, the venereal disease was caused by copulating too frequently, thereby stimulating the vagina to such a degree, as to make it take on an action capable of secreting this poison.

We know that when a stimulus is applied to any part, disproportioned to its excitability, that it will cause a morbid secretion. This is sufficient to account for the production of the disease, without having recourse to such a superstitious, and arbitrary explanation.

The venereal poison when once formed, is taken into the blood-vessels, and conveyed by this means to the different

parts, which we see affected with this disease; acting by its stimulating quality upon those, and in this manner producing the venereal affections, which we see in different parts of the body.

There are facts, which prove, beyond all kind of controversy, that this poison is taken into the blood-vessels.

An infected father, has communicated this disease to the *fœtus*, by the semen during a venereal act, the genital parts being in a state of perfect health. As a proof of this I beg leave to make the following curious quotations.

‘ Un dragon de la garde du roi d’Angleterre étoit affecté d’un ulcère syphilitique dans la gorge, qui résista longtems au mercure, pendant le traitement il cohabita avec sa femme, qui n’a jamais eu aucun mal syphilitique, et qui est encore eu parfaite santé. L’enfant qui fut le fruit de ce coït fut attaqué quelques semaines après la naissance, d’un ulcère syphilitique, à la gorge, dans le même endroit ou le père avoit le sien.

J’ai vu le père et le garçon, ils sont maintenant tous les deux parfaitement guéris.’*

‘ Je suis informé qu’il y a actuellement une famille régnante en Europe, dont tous les enfans sont nés avec le germe vérolique dans leur corps, et dont aucun a survécu jusqu’à ce qu’on se décidât à la fin d’administrer le mercure à l’animal qui fournissoit le lait pour la nourriture du dernier né.’

These are positive facts, that the venereal poison as well as mercury are taken into the circulation.

There are several cases related of the disease being communicated by transplanting teeth from apparently sound persons.†

Besides these, there are numerous and well established facts, of small-pox being communicated to the *fœtus in utero*, from the mother labouring under this disease. And may not this be the reason, why some persons cannot have the small-pox communicated to them after their birth.

* Voyez Swediaur. Tom. 2. p. 10.

† Ibid.

‡ See Foot on the venereal, 4to p. 472.

Children have been born with small-pox, who have received it in consequence of their mothers labouring under the same disease, a short time before their birth, and the matter obtained from the pustules of the new-born children, has communicated the disease.*

This is sufficient to shew that poisonous substances are taken into the blood-vessels, and conveyed by them to different parts of the body.

An ingenious opinion has been given upon the action of the venereal poison by a Dr. Barthez † upwards of twenty years ago. He supposes that a morbid action, similar to that excited by the syphilitic virus upon the genital parts, is produced in other parts, (as the throat) simply by sympathy, without the virus being immediately applied; (and that mercury cures this disease by exciting a new or different action, in consequence of which the disease is cured or suspended) ‡

For sympathy to exist any where but between minds, is a phenomenon truly astonishing!

If this theory were true, the sympathetic action ought certainly to exist in the throat, always while the genital parts are affected; but this is rarely, if ever the case.

According to this theory, the pains in the bones, the venereal nodes, &c. all ought to exist when the genital parts are most affected; but these affections we never see take place until the disease has existed several months, or even years; and almost always when the genital parts are perfectly cured, or at least, the disease has entirely disappeared from them. If this theory were true, I would ask, why we never see venereal ulcers first originating in the throat, in the mamma, or in other parts of the body, produce by sympathy venereal affections in the genital parts; whereas, if this were the case these affections ought uniformly to take place? Sympathy is

* See Foot on the venereal, quarto, p. 468.

† Voyez nouveaux élémens de la science de l'homme. Montpellier 1778, and Swediaur. Tom. 2.

‡ Mr. John Hunter has published this opinion as his own; whether it really belongs to him or to Dr. Barthez, the candid and unprejudiced reader will please to determine.

always reciprocal. On the contrary, we see the different venereal affections never are so.

Is it possible that this poison should act only by exciting a sympathetic action, between the genital parts and the skin the roots of the hair, the throat, and the bones, while some other parts (which every body would think more capable of taking on a sympathetic action,) should remain entirely unaffected by this disease?

I would ask why, after having destroyed the venereal virus at its source; after having cured the original venereal ulcers, we should still see syphilitic affections in other parts of the body?

We have seen positive proofs that the poisons of syphilis and small-pox, are taken into the blood-vessels, which is sufficient to explain why different parts of the body are affected with this disease.

Nature never uses two different ways when her work can be effected by one. My preceptor, Dr. Rush, has taught us to believe that all forms of disease are produced by the application of a stimulus disproportioned to the excitability of the part, to which it is applied. In short, to believe in the existence of a sympathetic affection, between the different parts of the body, where there can be no mind, is as erroneous as—

‘——The poor Indian whose untutored mind,
Sees God in clouds or hears him in the wind.’

AN EXPERIMENTAL INQUIRY INTO THE MODUS OPERANDI OF MERCURY, IN CURING THE LUES VENEREA.

‘ Ici la faiblesse de mon entendement m’arrete.’

BLANCHET.

I AM now entering upon a subject, which to treat it as it deserves, is a task truly difficult.

The manner in which mercury acts in curing the venereal disease, has always been one of the most inexplicable phenomena in the science of medicine.

As difficult as it is, physicians have not been deterred from forming many ingenious and beautiful hypothetical theories upon its action, in curing this disease.

To say that mercury cures the venereal disease by its weight, or by exciting a counter irritation to that produced by the venereal poison, or by producing a sympathetic action ; is saying no more than would apply to almost any article in the *materia medica*. Would it not be more rational to say, that the venereal poison affects the lymphatic system more than any other part of the body ; and that in those vessels it produces morbid excitement, in the same manner as any other stimulus produces morbid excitement in the blood-vessels. Now mercury acting upon the lymphatics, depletes in the same manner, as the lancet does in removing morbid excitement from the blood-vessels. In short, mercury is to the lymphatics what the lancet is to the blood-vessels.

The following experiments were made in order to ascertain the manner in which mercury acts in curing the Venereal Disease.

FIRST SERIES OF EXPERIMENTS.

EXPERIMENT I.

I inoculated my fellow-student, Mr. Wootton, on the right arm, with syphilitic matter, which was obtained from a chancre about an hour before : the part gradually inflamed, and in the space of four days, a complete chancre was formed.

EXPERIMENT II.

At the same time, and with a portion of the same syphilitic matter, mixed with an equal quantity of calomel, I inoculated Mr. Wootton on the left arm. There was no chancre, nor the least inflammation produced.

At my request, my friend Mr. Lee made the two following experiments for me.

EXPERIMENT III.

A girl about eighteen years old, was inoculated with a portion of small-pox matter, intimately mixed with calomel. It did not prevent her from having the disease in the usual time and form.

EXPERIMENT IV.

Two children were inoculated with the matter of small-pox, mixed with a gummous solution of mercury in the proportion of one drachm of quicksilver, and two of gum Arabic. They both had the disease in the usual time and manner.

EXPERIMENT V.

I inoculated myself with pure venereal matter * mixed with an equal quantity of a solution of corrosive sublimate of the strength of one scruple dissolved in an ounce of water. There was no chancre produced.

It is certain from the preceding experiments, that mercury being mixed with small-pox matter, does not prevent that disease from being communicated, but by mixing it with venereal matter, seems to have a contrary effect.

To ascertain how small a portion of the syphilitic virus would communicate this disease, and to see how small a portion of mercury would prevent it from being taken, I made the two following experiments.

EXPERIMENT VI.

I dissolved about the sixth part of a grain of syphilitic virus (obtained from a large venereal ulcer) in one ounce of water. I introduced about a fourth of a drop of this solution into the right arm. There was no chancre nor the smallest inflammation produced.

EXPERIMENT VII.

Into the above mentioned solution I dissolved half a grain of corrosive sublimate, and inoculated the same person, on the left arm; at the same time, and with the same quantity as mentioned in the preceding experiment. Neither chancre nor the least inflammation was produced.

EXPERIMENT IX.

I inoculated George Tavener, in the right arm with some of the venereal matter, obtained from the same chancre as mentioned above. A little inflammation was produced.

* This matter I knew to be pure, as it had produced a chancre in two persons who were inoculated with it.

EXPERIMENT IX.

At the same time, and with some of the same matter, mixed with tartar emetic, George Tavener was inoculated on the left arm. Result the same as in the preceding experiment.*

Mr. Shaw informed me that the person upon whom these two last experiments were performed, took at this time, (while the inflammation existed) a purge of calomel and jalap. Could this have prevented a chancre from taking place?

My friends Dr. Arthur, and Alexander May, assisted me in making experiments with some of the same matter as mentioned in experiments 6, 7, 8 and 9. A chancre was produced in none of their experiments.

In Cullen's *materia medica*, under the article mercury, we find the following experiment related.

A physician took a quantity of matter from a venereal chancre, and mixing it with a quantity of Plenck's gummy solution of mercury, he applied this mixture to a sound person, but could not find that it produced chancre or any other syphilitic symptom.' Dr. Cullen thought it very probable that the gummy solution might render the mercury inert.

* The matter with which these four last experiments were made, was obtained from a patient who had been labouring under the disease but a few days, and it appeared to be in a violent stage, as in that short time almost all the prepuce had sloughed off. With this matter a great many experiments were made, and in no one case did it produce chancre when with or without mercury. This I suspect was owing to the patient's having had applied a poultice to the part affected, which might have made the sore change its nature, while it was under the influence of the poultice, so as not to secrete venereal matter, but on the contrary, only pus, as any other common sore. We can as easily conceive that this may take place, as a patient labouring under a free flow of saliva, (in consequence of taking mercury) should cease upon the remission of a paroxysm of fever, or upon being seized with a diarrhæa. I have frequently seen salivation alternate with the paroxysms of fever, at the city hospital, in time of the yellow fever of 1799, and twice with a diarrhæa in the same place.

To see if there were any foundation for the Doctor's conjecture, I made the following experiment:

EXPERIMENT 10.

I inoculated a person with pure venereal matter, mixed with an equal quantity of a solution of Gum Arabic, in the proportion of two drachms of Gum Arabic to 8 ounces of water. It did not prevent a chancre from being produced.

The following experiment has been made by a Dr. Harrison.* He took the matter of an ulcer evidently syphilitic; and after having triturated it with the gummous oxide of mercury, he tried to inoculate for the syphilis with this mixture. The result was, that no infection was communicated. While the inoculation made with some of the same matter, not being mixed, produced an ulcer, and syphilitic symptoms.

Mr. Bell mentions, that 'the matter of venereal sores, when mixed with water used for washing them, has in various instances been swallowed by mistake; but we have no instances of pox being produced by it.'†

Mr. Hunter mentions the case of 'a gentleman who had chancres which discharged largely, used to wash the parts in milk and water, in a tea-cup with some lint, and generally let the lint lie in the cup with the milk. A little boy in the house, stole the milk and drank it: but whether or not he swallowed the lint, was not known. Nothing happened that could give the least suspicion of his having been affected either locally in the stomach, or constitutionally.'‡

Mr. Hunter also mentions, that 'a gentleman had a most violent gonorrhoea, in which both the inflammation and discharge were remarkably great. He had also a chordee, which was very troublesome at night; in order to cool the parts, and keep them clean, he had a bason of milk by the bed-side, in which, when the chordee was troublesome, he got up and dipped, or washed the penis. This operation he frequently repeated in the night. Under such complaints, he

* Voyez Swediaur derniere Edition. Tome 2, p. 245,

† See Bell on the Lues Venerea.

‡ See Hunter on the Venereal.

allowed a young lady to sleep with him ; her custom was, to have by her bed-side, a bason of tea to drink in the morning before she got up ; but unfortunately for the lady, she drank one morning the milk instead of the tea ; but nothing uncommon happened, for at least many months.'

The Abbé Fontana, informs us, that large quantities of the venom of the viper may be taken into the stomach without producing any deleterious effects ; but that the smallest particle of it when taken into the blood-vessels, produce death.

Why poisons should affect one part of the body, producing the most deleterious effects ; and the same poisons when applied to other parts of the body much more sensible, should remain inert, has never been satisfactorily explained.

All secreted poisons appear to be perfectly innocent when taken into the stomach ; but when taken into the blood-vessels, produce disease or death.

To ascertain if the gastric juice had any power of decomposing or rendering those poisons inert, the three following experiments were made.—

EXPERIMENT XI.

I kept fasting 24 hours, a young dog, apparently in good health ; from his stomach I obtained a small quantity of gastric juice, a portion of which, being intimately mixed with the same quantity of pure syphilitic matter, was introduced into the left arm of a young negro lad ; it did not prevent a chancre from being formed in the space of three days.

EXPERIMENT XII.

At the same time, a similar experiment was performed upon another person with the same result.

EXPERIMENT XIII.

A portion of small-pox matter was intimately mixed with anequal quantity of the gastric juice, (as mentioned in the

two preceding experiments) with this mixture two children were inoculated, both of whom had the small-pox in the usual manner.*

We see by the preceding experiments, that the gastric juice, when mixed with the poisonous secretions of syphilis and small-pox, has no power of decomposing or rendering them inert, at least, when out of the stomach.

Poisons not producing their effects when taken into the stomach can only be explained upon the principles of Dr. Rush.†

EXPERIMENT XIV.

I inoculated on the right arm a man with venereal matter mixed with an equal quantity of a solution of the sulphate of copper, of the strength of one scruple to an ounce of water. It did not prevent a chancre from taking place.

EXPERIMENT XV.

At the same time, I inoculated the same person on the left arm with venereal matter, mixed with an equal quantity of a solution of the sulphate of iron, of the strength of one scruple to an ounce of water. It did not prevent a chancre from taking place.

EXPERIMENT XVI.

At the same time, I inoculated the same person on the left arm with some of the same matter mixed with an equal quantity of the oxyde of mercury, or precipitate *per se*. It prevented a chancre from taking place.

It is evident from the preceding experiments, that when mercury comes in contact with the syphilitic virus, it either

* These three last experiments were made as soon as possible after the gastric juice was obtained from the stomach.

† He has ingeniously divided the body into seven systems; upon each of which, all the different kinds of stimulants appear to have some particular action; but are by no means more confined to them than the different *forms* of disease are.

renders it entirely inert, or changes its nature in such a manner, as to render it incapable of producing the venereal disease.

We can as easily conceive this to be the case, as that an acid should destroy an alkali, or render it inert. We may as readily believe that an elective attraction should take place between the venereal matter and mercury, as between nitric acid and mercury, or any other agents used in chemistry.

It will not be improper here to take notice of an opinion which has been lately offered to explain the *modus operandi* of mercury, in the cure of the venereal disease. The opinion alluded to, is this : that the anti-venereal properties of mercury depend entirely upon the oxygene which is contained in its different preparations. It is said, that mercury is absorbed and carried into the blood-vessels, in the form of oxide, or mercurial salt, and that the mercury leaves the oxygene and passes off under its metallic state, as a foreign body, from the mass of blood, by perspiration or other excretions. *

The arguments that have been adduced in support of this opinion, are at first sight plausible : but they can neither stand the test of reason nor experiment.

It cannot be the oxygene contained in the preparations of mercury, that cure the venereal disease ; because we take into the blood-vessels more oxygene in a few inspirations, than is contained in a quantity of the muriate of mercury sufficient to cure a dozen cases of syphilis. Besides, the blue pill and mercurial ointment, are not in a state of oxide, as is satisfactorily proved by the experiments of Mr. Lee.†

We have seen that the poisons of syphilis and small-pox, are taken into the blood-vessels. The same fact is equally true, when applied to mercury.

Our not being able to see those substances in the blood-vessels, is no proof of their not existing in them.

If mercury was not taken into the blood-vessels, how could it cure venereal ulcers, and other external syphilitic affections?

* Voyez Swediaur, tome 2.

† See Medical Repository, vol. 4.

Bodeloque,† informs us that mercury has been detected in the liquor amnii of a woman, who had taken large quantities of it.

A piece of gold will be turned white by rubbing it on the body of a person that is salivated.‡

Watches, rings, or other gold jewels, will begin to whiten from the moment that mercury enters the mass of blood; or when it begins to pass off by perspiration.|| Now I will thank any person to inform me, how this can be effected without mercury being taken into the circulation.

Swediaur§ informs us, that a Gilder in Paris was salivated by the fumes of mercury, to such a degree, that his legs and thighs were very much swelled, and there came upon them a great many blisters, which discharged an abundance of thick serous water, which was kept in pots. After a certain time there was deposited in them globulus of mercury.

We are informed by numerous authorities, and those too the most respectable, that mercury in its crude state, has been found in different parts of the body.

In order to prove that mercury can exist in the blood-vessels, the following experiments were made.

SECOND SERIES OF EXPERIMENTS.

EXPERIMENT I.

I injected into one of the jugular veins of a small dog, one ounce of quicksilver :—Immediately after its introduction, the action of the heart and arteries appeared to be increased, attended with a respiration somewhat laborious. He appeared rather dull for two or three days, but gradually recovered his

† See Bodeloque's Midwifery, vol. 1.

‡ See Flack on Mercury.

|| Voyez Swediaur, tom. 2.

§ Ibid. tome. 2, p. 369.

former activity, and appears now to be in perfect health. It is more than three weeks since this experiment was performed.

EXPERIMENT II.

One *scruple* of calomel with three drachms of pump water were injected into one of the jugular veins of a puppy. In eleven minutes after it was introduced, he tried several times to vomit. In forty minutes he discharged fæces—in forty-five there was a great secretion of saliva—in fifty minutes a hæmorrhage from the nose—and in sixty he died.†

EXPERIMENT III.

I injected into one of the jugular veins of a bitch, *half a drachm* of precipitate *per se* with three drachms of pump water. She died in sixteen minutes. She had no secretion of saliva.

EXPERIMENT IV.

I injected into one of the jugular veins of a dog, eight grains of calomel, with three drachms of pump water.

In 30 minutes he puked a great deal, and in 60 he discharged fæces. There was a very considerable secretion of saliva, which continued until he died. It had not the mercurial fetor. He died in 17 hours.

EXPERIMENT V.

I injected into one of the jugular veins of a small bitch, four grains of calomel with two drachms of pump water. She died in thirty-six hours. There was no secretion of saliva.

EXPERIMENT VI.

I injected into one of the jugular veins of a dog, two grains of calomel with two drachms of water. It did not produce death. He appeared to suffer very little from the experiment. He now appears to be in perfect health.

† My fellow student, Mr. Rowan, was present at the two last mentioned experiments.

EXPERIMENT VII.

One grain of calomel, with one drachm of pump-water, were injected into one of the jugular veins of a dog. Result the same as the preceding experiment.

It is evident from these experiments, that both crude mercury and calomel can exist in the blood-vessels.

Is it not presumable, that much larger quantities of calomel can exist in the blood-vessels, when it is gradually taken into them by the lymphatics, by which means the stimulus is more equable, and of consequence they are not so violently stimulated as when it is suddenly introduced by means of injection.

EXPERIMENTAL PROOFS THAT THE LUES VENEREA, AND GONORRHOEA, ARE TWO DISTINCT FORMS OF DISEASE.

It is an unfortunate truth, and much to be regretted, that most physicians, had rather theorise than experiment.

All opinions in medicine, in order to be lasting, ought to consist of the pure and simple expression of facts.

It has been a subject of much debate and is still the case at present, whether the lues venerea and gonorrhoea are one and the same, or two distinct forms of disease.

The arguments that have been adduced to prove that the syphilis and gonorrhoea are the same disease, are so well known, that it would be unnecessary to introduce them in this place.

The following experiments, compared with the preceding, it is hoped, will be sufficient to establish the difference between the secretions of syphilis and gonorrhoea.

THIRD SERIES OF EXPERIMENTS.

EXPERIMENT I.

I was inoculated by Dr. Barton, on the right arm with gonorrhoeal matter.* There was no inflammation produced.

* This matter the Doctor obtained from one of his patients; he thought it was perfectly virulent.

EXPERIMENT II.

I was inoculated by Dr. Barton, at the same time, and with some of the same gonorrhoeal matter, mixed intimately with calomel. There was no inflammation produced.

EXPERIMENT III.

My fellow student Mr. Rowan, was inoculated in the right arm with some of the gonorrhoeal matter, as mentioned in the two preceding experiments. There was not the least inflammation produced.

EXPERIMENT IV.

At the same time and with some of the same gonorrhoeal matter, mixed with an equal quantity of gum camphor, Mr. Rowan was inoculated on the right arm. There was no inflammation produced.

EXPERIMENT V.

I inoculated my friend Mr. Thomson, on the right arm with gonorrhoeal matter. There was no inflammation produced.

EXPERIMENT VI.

At the same time Mr. Thomson, was inoculated with some of the same gonorrhoeal matter, mixed with an equal quantity of a gumous solution of mercury, of the following proportions. Quicksilver sixteen grains, gum arabic two scruples, water eight ounces. There was no inflammation produced.

EXPERIMENT VII.

I inoculated a servant man, on the right arm with gonorrhoeal matter just obtained from the penis. There was no chancre nor the least inflammation produced.

EXPERIMENT VIII.

At the same time, I inoculated the same person, (as mentioned in the preceding experiment) on the left arm with gonorrhoeal matter mixed with calomel. There was no chancre nor the smallest inflammation produced.

EXPERIMENT IX.

Three weeks from the time, when the experiment was performed upon me by Dr. Barton. I was inoculated with some fresh gonorrhoeal matter on the right fore arm. There was no chancre nor the least inflammation produced.

EXPERIMENT X.

At the same time, and with some of the same matter mixed intimately with a weak solution of nitric acid, I inoculated myself on the left fore arm. There was no chancre formed; but some little inflammation, caused, I suppose, by the acid, which soon went off.

EXPERIMENT XI.

I inoculated Mr. Rowan * on the right fore arm with some of the gonorrhoeal matter, as used in the two last mentioned experiments. There was no inflammation produced.

EXPERIMENT XII.

At the same time, and with some of the same gonorrhoeal matter, as used in the preceding experiment, mixed with a weak solution of lunar caustic, I inoculated Mr. Rowan on the left fore arm. There was no inflammation produced.

EXPERIMENT XIII.

I inoculated Mr. Thompson, a student of medicine, a second time (ten days after the first experiment was performed upon him) on the right arm with some of the gonorrhoeal matter, as used in the preceding experiment. There was no inflammation produced.

* This experiment was made two weeks after the third experiment.

EXPERIMENT XIV.

At the same time I inoculated Mr. Thomson on the left arm, with some of the same gonorrhoeal matter, mixed with Gum Arabic. There was no inflammation produced.

EXPERIMENT XV.

Two pieces of lint well soaked in fresh gonorrhoeal matter, (which I was informed was perfectly virulent) were applied behind the glands penis under the prepuce, and suffered to remain two days and a half. There was no chancre, nor the least inflammation produced.

EXPERIMENT XVI.

With fresh gonorrhoeal matter (which I was informed was perfectly virulent) an healthy young man was inoculated on the glands penis, and upon the prepuce. No chancre nor the smallest inflammation was produced.

It is evident, from comparing these experiments, with those made with the venereal virus, that the two secretions are entirely different. We saw in the experiments made with the venereal virus, that chancre was produced, in almost every case, whereas the experiments made with gonorrhoeal matter were incapable of producing even inflammation.

Experiments must overthrow the illusions of opinion, the whims of caprice, and confirm and establish the decisions of nature.

IT APPEARS FROM THE FOLLOWING EXPERIMENTS, THAT DOGS, &c. ARE NOT CAPABLE OF BEING INFECTED WITH THE VENEREAL DISEASE.

EXPERIMENT I.

I inoculated a dog, on the under side of each ear, with venereal matter; and also applied a portion of the same matter to each eye. There was not the smallest inflammation produced, either in his eyes, or on the under side of the ears.

EXPERIMENT II.

I inoculated a cat, with venereal matter, there was no inflammation produced.

EXPERIMENT III.

A large portion of venereal matter, was dissolved in two drachms of water, and injected into the vagina of a bitch. It did not produce the disease.

EXPERIMENT IV.

I inoculated a dog, on the inner side of each ear, with gonorrhoeal matter, and also applied some of the same matter to each eye. There was no inflammation produced, either in his eyes or on the under side of his ears.

EXPERIMENT V.

I dissolved a large portion of gonorrhoeal matter, in water, so much so, that it was quite turbid; and injected it into the eyes and nostrils of a cat, apparently in perfect health. Her

eyes appeared inflamed soon after ; but it went off in two or three hours.

The cat on which this experiment was made, appears not to have suffered the least inconvenience from the experiment.

EXPERIMENT VI.

I inoculated another cat, on the under side of each ear, with gonorrhoeal matter. There was not the least inflammation produced.

In addition to these experiments, Mr. Hunter informs us that ' he has repeatedly soaked lint in matter from gonorrhoea, chancre, bubo, and introduced it into the vagina of bitches, without producing any effect. I have introduced it into the vagina of asses without any effect. I have introduced it under the prepuce of dogs, without any effect. I have also made incisions, and introduced it under the skin, and it has only produced a common sore. I have made the same experiment upon asses, with the same result.*'

M. de Paw,† informs us that the dogs which the Spaniards brought into the different islands, and districts of the new world, were very soon seized with the venereal disease. He also says the dogs of Peru, now-a-days experience the approaches of this disease.

From the experiments made by Mr. Hunter and myself, it is satisfactorily proved, that those animals cannot be affected with the venereal disease ; and we must believe M. de Paw is as much mistaken on this subject, as he is concerning the origin of this disease.

Before closing this essay, I cannot forbear mentioning that the difficulty of the subjects and the time in which they were written, has unavoidably prevented the preceding pages from being handled in such a satisfactory manner as could be wished. Those who read to find fault, will not be disappointed ; but those who are more candidly disposed, will at least say, that I have attempted to investigate an obscure part of the science of medicine.

* See Hunter on the Lues Venerea.

† Voyez Recherches Philosophiques sur les Americains. Tome premier.

Each of the Professors of the University of Pennsylvania will please to accept my thanks for the information I have received from their lectures.

My former preceptor, Dr. Beanes, will also please to accept my best respects for the many attentions I have received from him.

I beg leave in this place to return my best thanks to Dr. Physick for the information I have received from his valuable lectures upon the operative part of surgery.

FINIS.



